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The MONTHLY REVIEW

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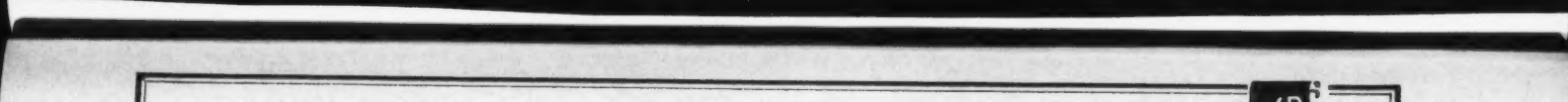

The MONTHLY REVIEW

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MONTHLY PROGRESS REPORT

of P. W. C. Strausser,
Research Associate of the American Electro-Platers' Society

1. *Porosity Tests.* — My principal activity during the month was the study of tests for porosity of plated coatings. The ferroxyl test, which is very useful for detecting pores in copper, nickel or chromium coatings on steel, was fully described in previous reports. It is not applicable, however, to deposits on zinc, or to deposits which include a layer of zinc.

(a) *Porosity of coatings over zinc.* — The method used depended upon immersing the specimen for 30 seconds in a solution that was $\underline{N}$ in sulphuric acid (49 g/L H_2SO_4) and 0.08 $\underline{N}$ in copper sulphate (10 g/L $CuSO_4.5H_2O$). Copper is deposited by immersion on any zinc that is exposed through pores. If the latter are very fine, the presence of the deposited copper can be more readily detected if the specimens are next immersed in water saturated with hydrogen sulphide. This produces black spots of copper sulphide.

This method was applied to composite coatings consisting of (a) zinc, nickel; (b) zinc, nickel, chromium; (c) zinc, copper nickel; (d) zinc, copper, nickel, chromium; and (e) zinc, chromium. In all cases any large pores or unplated areas were readily revealed, but the detection of very minute spots was erratic and uncertain. Just as with the ferroxyl test, it is difficult to express the porosity on any numerical scale.

(b) *Porosity of nickel over copper.* — By simply immersing the specimens in hydrogen sulphide water, black spots were produced wherever any appreciable area of copper was exposed, but fine pores were not readily detected.

(c) *Porosity of zinc coatings on steel.* — An effort was made to detect pores in zinc coatings on steel by making the specimens anodic in a 0.5 $\underline{N}$ solution of sodium hydrosulphide. This was

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made by saturating 0.5 N sodium sulphide (40 g/L Na_2S) with hydrogen sulphide. The anodic treatment was continued for 5 minutes at 25°C (77°F), using 6 volts. Where large areas of steel were intentionally exposed, they turned black. It was difficult, however, to detect pores consistently in even thin zinc coatings (0.0002 inch), much less in thick deposits. The method is therefore of doubtful value.

(d) *Porosity of cadmium coatings on steel.* — It was reported by Clarke that pores in cadmium coatings on steel can be detected by immersing the specimens for 10 minutes in a 1 per cent solution of hydrochloric acid at 20 to 23°C (68 to 73 F). Numerous tests made here yielded erratic results, and it was sometimes difficult to distinguish between hydrogen bubbles and air bubbles. Another disadvantage of this test is that it does not furnish any permanent record of the porosity.

It is therefore difficult to detect pores in either zinc or cadmium coatings. It is at least doubtful whether small pores have any effect on the protective value of such coatings though larger exposed areas of steel may accelerate the corrosion of the coatings. The fact that the results of exposure tests, especially in New York and Pittsburgh, showed no effects of the conditions under which the zinc or cadmium were deposited, makes it improbable that porosity of these coatings is an important factor.

2. *Miscellaneous Activities.* — Owing to the fact that I am now completing minor details of the study of plated steel, it was possible for me to assist in other phases of this investigation, including the following:

(a) *Preparation of exhibits.* — I re-covered the exhibit cards for use in the A. S. T. M. conferences and in lectures given by Dr. Blum and Mr. Hogaboom.

(b) *Compilation of records.* — I assisted in the compilation of the reports of inspections, to be used in the complete report now being prepared by Dr. Blum.

(c) *Overhauling of equipment and supplies.* — The plating and buffing supplies were rearranged and inventoried. Arrangements were made to have all the electrical and ventilating equipment tested and repaired where necessary.

(d) *Test for trivalent chromium.* — A few tests were made on a commercial device, not yet marketed, for the colorimetric determination of trivalent chromium, and suggestions for its improvement were informally transmitted through Dr. Blum.

THE REDUCTION OF MIST OR SPRAY FROM CHROMIUM BATHS BY FIXED OILS SUCH AS FISH OIL[1]

By C. M. Alter[2] and F. C. Mathers[3]

Read at Chicago Convention, 1933

ABSTRACT

SMALL quantities of liquid organic oils, such as fish oil, belonging to the chemical division of "fixed oils, fats and waxes"[4] were found to be exceptionally effective in reducing the quantity of spray or mist from chromium baths. The important thing about these oils is that they produce a foam on the surface of the bath and that they are not rapidly oxidized or destroyed by chromic acid. This invention[5] eliminates most of the loss of electrolyte caused by spray or "throw out" as well as improves the health conditions in the plating room.

REVIEW AND INTRODUCTION

The mist of fog or spray produced by the gases evolved from a chromium bath causes ill health to the workman, loss of bath, and ventilation difficulties. A large proportion[6] of all workers in a chromium plating room is rather seriously injured by the spray.

Any evolution of a gas from any solution produces a spray. Hence, acid pickling of iron and the recovery of copper, zinc or other metal by electrolysis using insoluble anodes and the charging of storage batteries cause troubles with mist or spray similar to those encountered in chromium plating except that the chromium spray is much more toxic and corrosive. Many remedies have been suggested especially for acid pickling. In most cases some material is added which will produce a foam or froth on the surface of the solution. Some of these same materials are used also in the foaming types of fire extinguishers.

"Goulac" or sulphite paper liquor which is recommended as an addition agent in lead, zinc and cadmium plating is sold for use as a foam producing agent in the pickling of iron in acid and in foaming fire extinguishers. An alkaline infusion from extracted licorice roots is said to be used in some fire extinguishers.

Saponin is another foaming or frothing agent. Patents[7]

1. Contribution from the Chemical Laboratory of Indiana University, Bloomington.
2. Research fellow at Harvard.
3. Professor of Chemistry at Indiana University.
4. Allen, Commercial Analysis. (Blakiston) 5th Ed., Vol. II.
5. A patent application has been filed.
6. Schwartz and Seike, Zentr. Gewerbehyg. Unfallverhütung, **17**, 232 (1930), through *C. A.* **25**, 1741. J. State Med., **40**, 696 (1932)
7. Taiton, Fr. 674, 467 (May 2, 1929), Can. 303,359 (Aug. 26, 1930), and U. S. 1,839,905 (1932).

have recently been issued and applications have been filed for other patents covering foam or froth producing materials for use in electrolytic baths with insoluble anodes. Some of the foam producing substances claimed in the patents are: cresylic acid, wood and coal tar, xylenol, creosote, rubber latex, saponin, and gum arabic and sometimes with silicon dioxide and silicic acid.

Chromium baths, which are vigorously oxidizing and reactive with most materials of an organic nature, very quickly destroy the activity of the above foaming agents. Therefore, anti-spray materials of much less chemical activity are necessary for chromium plating baths.

Saturated hydrocarbons, of which the mineral oils such as kerosene, paraffine, etc. are examples, have long been known to be unacted[8] upon by chromic acid. Most of the anti-spray materials[9] for chromium plating baths use saturated hydrocarbons similar to kerosene. In most cases, a layer of gasoline, kerosene, paraffine oil or a coal tar hydrocarbon or some artificial or hydrogenated hydrocarbon is allowed to float on the top of the bath. A description of one experiment said that the layer of kerosene[10] was 0.25 in. (6.35 mm.) thick. This quantity of kerosene darkened the deposit of chromium and thereby injured its decorative value but did not hurt the quality of the chromium for mechanical uses. The spray was reduced to such an extent that the electrolysis was carried on without a hood or other means of forced ventilation. An alcoholic solution of clove oil which has been recommended[11] for a non-oxidizing bath using chromium chloride was of no value in the chromic acid bath.

A variation from the use of the oil layer (Kuppa) is the use of a layer of cork or of glass wool soaked in paraffine.

Serious objections have been raised to the use of these oil layers. Besides introducing a fire hazard some of these oils are toxic and themselves cause sores[12].

In most plating plants, exhaust fans[13] are depended upon to ventilate and remove the troublesome spray. The cost of such a power exhaust system is quite large. The sticky gummy deposit from the spray covers everything exposed to it and makes upkeep high.

8. Ludwig, Ann. Chem. (Liebig) **162**, 47 (1872).
9. Emmens, U. S. 501,996 July 25, 1893; Mond, Brit. 270,514, May 12, 1927; Kuppa, Metalwaren Galvano-Tech., 24, 408 (1926) Blasburg, Brit., 327,293 Apr. 8, 1929; J. Bauer, Brit. 327911; Rassow and Wolf, Chem. Ztg., **55**, 73 (1931).
10. Schneidewind and Urban, Trans. Am. Electrochem. Soc., **53**, 463 (1928).
11. Peffer and Pierce, U. S. 146 5173 (1923).
12. Wolters and Brandt, Metallwaren-Ind. U. Galvano—Tech., **30**, 206 (1932)
13. Blum and Bloomfield, Metal Ind. (N. Y.), **27**, 324 (1929).

The loss of electrolyte from this spray or "throw out" is estimated[14] to be equal to the "plate out" or the part of the chromic acid usefully used. Although it is true that the cost of material is one of the smaller items contributing to the cost of chromium plating, any possible reduction in material wasted is highly desirable.

THEORY

The data which follow show that spray can be eliminated or greatly reduced by the use of certain oils on the surface of the bath. The mineral oils form a film on the surface of the bath. The gas evolved during electrolysis passes through this film but does not carry with it a large amount of electrolyte. This effect is due in some measure to the low surface tension of the oil film. Chromic acid of the concentration 25 g. per 100 cc. of water shows a surface tension of about eighty dynes per centimeter while that of a film of mineral oil is approximately half that value. When the gases liberated at the electrodes pass up through the solution and come to the surface of the bath the size of the bubbles of the gases that pass off into the atmosphere is dependent on the surface tension of the liquid, the greater the tension, the smaller the bubbles. The oil film or layer of foam has the effect of permitting the gas to pass from the bath in the form of a few large bubbles instead of the many small ones as is the case when no oil film is present. These few large bubbles have less surface area than the same volume of gas in the form of many small bubbles. The less the surface area of the bubbles of gas, the less liquid will be carried out of the bath by a given volume of gas.

The very excellent and persistent foam formed by the liquid fixed oils such as fish oil makes possible the elimination of spray without the use of such large quantities of oils as is needed in the case of kerosene and similar oils. It can be readily seen that a layer or blanket of foam on a bath is just as effective in allowing large bubbles of gas to escape as is the same thickness of kerosene and similar hydrocarbons which do not produce foam. Therefore, a very much greater total weight or volume of non-foaming oils is required to give a thickness equal to that of the foam from a much less quantity of any foaming oil.

EXPERIMENTAL

It was the purpose of this research to compare the various oils

14. Haring and Barrow, Tech. Paper, Bur. Standard, 346.
15. Alter and Mathers, Trans. Am. Electrochem. Soc. 56, 363 (1929) and Metal Clean. and Finishing, 1, 517 (1929).

in a quantitative way and to select the ones best suited for use on chromium plating baths for the reduction of spray.

Kind of Oil	Amt. of Oil Used	Condition on Surface of the Bath	Percent Reduction: "Throw-out" or "Spray Loss"	Comment
Light mineral	10 drops	film	95	
Heavy mineral	5 "	film	73	
Heavy mineral	10 "	film	97	dark deposit
Kerosene	5 "	Slight film	00	
Kerosene	20 "	film	46	
Kerosene	4 cc.	thin layer	85	dark deposit
Absorption oil	5 drops	film	80	
Absorption oil	10 "	film	93	
Absorption oil	5 cc.	thin layer	93	dark deposit
Flotation Oil	5 drops	slight film	slight	
Crude mineral	2 "	slight film	00	
Olive Oil	5 "	good foam	100	
Castor Oil	5 "	fair foam	97	
Oleic acid	5 "	fair foam	65	did not persist
Lanoline	3 "	light foam	90	
Pork lard	2 "	little foam	50	
Whale Oil	1 "	sufficient foam	100	
Whale Oil	5 "	excessive foam	100	
Cod Liver Oil	1 "	good foam	100	
Cod Liver Oil	5 "	excellent foam	100	
Fish Oil	1 "	good foam	100	
Fish Oil	5 "	excessive foam	100	
Sperm Oil	1 "	slight foam	30	
Sperm Oil	3 "	heavy foam	100	
Oil of Sesame	1 "	slight foam	30	
Oil of Sesame	5 "	excessive foam	100	
Various essential oils		no foam or film	0	oils oxidized by bath

The test baths used in this work were similar in composition to those used in commercial plating establishments and already described in an earlier paper[15]. Electrolysis was carried out in 150 cc. beakers containing approximately 100 cc. of electrolyte. The area of the surface exposed was about 2.5 sq. in (16 sq. cm.). Lead anodes and steel or copper cathodes were used.

It was necessary to devise a method of quantitatively determining the amount of spray lost from these baths. To do this the cells or beakers, all of the same size, were filled with the chromium bath to a uniform distance from the top of the beakers. A filter paper was then placed over each beaker in such a way that the liquid or spray which was thrown out of the bath would be caught and absorbed. After electrolysis for a given length of

time the filter papers containing the absorbed acid were ignited in weighed platinum crucibles. The weight of chromic acid lost from each bath was calculated from the weight of the chromium sesquioxide, Cr_2O_3.

Although these experimental conditions were different from those under which the commercial process is carried out, nevertheless all the results obtained in this research are comparable with each other. In all experiments, baths without oil were run in series with the baths carrying the various oils and since all other conditions were constant, the reduction of spray-loss was determined quite accurately in every case.

Preliminary experiments showed that excess oil (more than ten drops per square inch) caused a darkening of the cathode deposit. This was especially true with the mineral oils. The table shows the relative value of the various oils for the reduction of spray. The amount of oil given in the table is that used on the surface of a bath in a 150 cc. beaker, i. e., 2.5 sq. in.

The fish oil that was used was bought as "ordinary fish oil" and was probably from the Menhaden fish. It will be noticed that the oils which formed the best foams and therefore the ones which reduced the spray loss to the greatest extent contained glyceryl esters of the higher fatty acids. Cod liver oil, castor oil and olive oil functioned quite well but the foam did not persist as long as in the case with fish oil and whale oil.

CONCLUSIONS

I. Spray-loss from chromium plating baths can be reduced to zero or to almost that by the use of oils or oily substances which form a layer of foam on the surface of the bath.

II. Foaming materials are better than those that form only a film or layer because such a small quantity of this type of oil is needed that it does not impair the color of the deposit.

III. The following oils are the most satisfactory: fish oil, whale oil, sperm oil, oil of Sesame, olive oil, cod liver oil, castor oil, and lanoline (wool fat).

IV. Fish oil is recommended for the purpose of reducing spray-loss from electroplating baths. This recommendation is based on the fact that it functions as well as any of the above mentioned oils and because it is the least expensive of the group—now twelve cents per gallon.

V. It is recommended that one drop of fish oil be used per

square inch of surface of the bath. After several hours of electrolysis or when the quantity of foam becomes insufficient to prevent spray more oil must be added. Apparently these fixed oils are not attacked much by the bath.

VI. The oils, in the quantities recommended, did not seem, to the authors, to have any effect on the color or other properties of the chromium plate.

VII. It is not claimed that all ventilation will be unnecessary if these oils are used, but any ventilating system will have much less load to carry and, therefore, conditions in the plating rooms will be much improved.

VIII. The saving of solution, which is now lost in the spray, or "throw out" should compensate for the cost of the oil.

IX. These oils, especially fish oil, whale oil, etc., give a much more persistent foam than an equal weight of any of the other foaming agents recommended or patented for use in any kind of a solution where gas is being evolved. These other foaming agents cannot be used on chromic acid because of their rapid oxidation and destruction by the chromic acid.

RESOLUTIONS FOR CHANGING THE CONSTITUTION OF THE A. E. S.

No. 1 — *A resolution for changing Article 7 of Section* 1, *of our Constitution as follows*:

"This Society shall meet in Convention each year within twenty days before or ten days after July 1st of each year."

No. 2 — *Sections* 4 *and* 5 *of Article* 4 *of the Constitution*, "making the office of Secretary, Treasurer and Editor one; — that is, to have it made a one-man office and the salary figures to $300 per year, besides allowing for stenographic and clerical services as needed to faithfully and effectively carry on the duties of this office."

The reasons for these changes are: that by combining the offices of Secretary, Treasurer and Editor, it will concentrate the work to one place and save quite a little money and time, not only for the one holding this job but for the Branch Societies and Treasury as well, and instead of making the office for 1 year, it should be for 3 years. This change would necessitate changing Section 1 of Article 3 to read as follows: (after the words "Second Vice-President") — "a Secretary, Treasurer and Editor, and an Executive Board."

Proposed also that Sec. 3, Article 11 reducing the per capita tax of 35% to 20%.

T. C. EICHSTAEDT.

The Abstract Section

EDWARD B. SANIGAR

THESE abstracts are mostly adapted, by permission, from "Chemical Abstracts", the references to that publication being given in the form — *C. A.*, **27**, 3402 (1933) *i.e.*, Chemical Abstracts Volume 27, column 3402 in the year 1933. The name appearing at the end of the abstract is that of the abstractor. Abbreviations for scientific journals are those used by "Chemical Abstracts" (see *C. A.*, **25**, 6019 (1931)). E. B. S.

Platinum plating. ALAN E. W. SMITH. *Metal Ind.* (London) **43**, 201 (1933).—Corrosion tests on electroplated platinum were made as follows: Brass metal strips were silver-plated and then plated with platinum. These strips were used as laboratory spatulas for 5 months. The spatulas were then dipped into ammonium sulfide solution to test for broken coatings; none were found. Platinum plate is appreciably harder than nickel plate. Other test specimens were exposed to acid fumes, hydrogen sulfide, steam and gas-furnace fumes, 10% hydrochloric acid, a diluted Lysol solution, and out-of-doors atmosphere. After one month all these specimens showed some pitting; the one exposed to hydrogen sulfide was the worst. In all cases the platinum itself was not attacked. The basis metals were corroded because of pinholes in the deposit. Nickel, platinum and chromium were plated on steel specimens and subjected to the action of a 2% solution of iodine in alcohol. The nickel was eaten through in one day. In two days the platinum showed signs of pinholing, and the chromium signs of cracking at the edges. Silver- and platinum-coated reflectors were tested for resistance to temperature. The silver reflector failed below 300°C. and the platinum reflector failed between 400-500°C. Platinum reflects only 65%, while silver reflects 92%, of visible light rays. *C. A.* **28**, 1606 (1934).

Palladium. German patent No. 587,737. November 7, 1933. W. C. HERAEUS G.m.b.H.—Palladium is deposited as a durable plating by the electrolysis of a neutral or weakly acid solution containing an excess of chloride-ions over palladium at 20°C. and with a current of 1 amp./sq.dm. (9 amps./sq.ft.). Crude palladium may be used as the anode and sodium chloride solution as the anolyte if a diaphragm is used. The electrolyte may consist of a solution of sodium-palladium chloride (Na_2PdC1_4) and sodium chloride. *C. A.* **28**, 1610 (1934).

Metal plating. German patent No. 585,914. October 12, 1933. Vereinigte Isolatorenwerke A.-G.—Pressed objects made from phenol-formaldehyde condensation products generally, with filling materials and dyes optionally incorporated, are given a coating of silver salt solution, such as silver nitrate, without addition of an additional reducing agent. *C. A.* **28**, 1328 (1934).

Silvering glass surfaces. U. S. patent No. 1,942,686. January 9, 1934. G. F. COLBERT and W. H. COLBERT (to Liberty Mirror Works).—For

partially silvering a glass surface, a coat of silver is precipitated over the area, a film of copper is electrodeposited on the silver, a pattern resistant to the action of a solvent of these metals is superposed on the coatings and the exposed parts are then subjected to the action of a solvent, such as an acid solution. Various details and modifications of procedure are described.

C. A. **28,** 1831 (1934).

Silver alloys. Canadian patent No. 338,197. December 26, 1933. D. GRAY, R. O. BAILEY and W. S. MURRAY (to Oneida Community, Ltd.).—Tarnishing of articles formed of silver or of alloys containing silver is retarded or inhibited by electrodepositing indium on the article and thereafter diffusing the indium through the silver by the application of heat.

C. A. **28,** 1654 (1934).

Case for holding silverware to prevent tarnishing. U. S. patent No. 1,939,497. December 12, 1933. G. E. HERRING.—Details are described of a case, surfaces of which are treated with a sulfide-neutralising chemical, such as lead acetate. (cf. *C. A.* **27,** 4692). *C. A.* **28,** 1488 (1934).

Electrodeposition of metals. British patent No. 398,759. September 21, 1933. C. E. YATES (to Anaconda Copper Mining Co.).—Sheet copper, etc., is produced continuously by deposition on a partially immersed cathode drum. Between stripping and re-immersion the cathode surface is subjected to the action of a pad comprising graded abrasive articles, e.g., of silicon carbide, embedded in a soft rubber compound, the rubber being in amount sufficient to exert a smoothing action, e.g., abrasive 20, rubber 80 parts before vulcanisation. *C. A.* **28,** 1609 (1934).

The gray coloring of copper alloys by pickling in arsenic. GEORG GROSS. *Oberflachentech.* **10,** 267 (1933).—The process of gray coloring of copper alloys used for parts of instruments by immersion in a solution of 83g. arsenic trioxide (As_2O_3) and 83g. ferrous sulfate ($FeSO_4$) in 1000g. of commercial hydrochloric acid was investigated with respect to an eventual improvement in composition and quality of product. The solution can be applied without having been first heated and cooled again. The coloring velocity increases with temperature, but 45°C. should not be exceeded for a steel-gray color. Instead of the commercial acid, chemically pure hydrochloric acid can also be used. Coloring can be accelerated by repeated rinsing and drying of the articles; the gray tone can be obtained more easily on matte surfaces than on polished. The rapidity of pickling can be increased 50% by using 100g. hydrochloric acid, 21g. ferrous sulfate crystals ($FeSO_4.7H_2O$) and 14g. arsenic trioxide, at room temperature. A solution of 11.2g. ferrous sulfate crystals and 10.3g. arsenic trioxide in 100g. hydrochloric acid reduces coloring time from 25 minutes to 10 seconds; it can be used for zinc sheet, brass, tombac, red brass, cast bronze, rolled bronze and copper sheet to obtain gray tones. *C. A.* **28,** 1314 (1934).

Nickel anodes. U. S. patent No. 1,941,256. December 26, 1933. G. F. GEIGER (to International Nickel Co.).—Rolled or cast nickel anodes containing 0.002-0.0075% sulfur and containing oxygen corrode with smooth surfaces and substantially no anode sludges. U. S. patent No. 1,941,257 relates to a similar addition of sulfur in small proportion to nickel-bearing material before pouring. *C. A.* **28,** 1282 (1934).

Chromium plating. French patent No. 754,360. November 6, 1933. SIEMENS and HALSKE A.-G.—Black chromium coatings are obtained by electrolysis with a current density of 10-20 times the normal c.d., i.e., about 10,000-20,000 amp./sq.meter. (1,000-2,000amp./sq.ft. E.B.S.). The electrolyte may contain acetic acid. *C. A.* **28,** 1282 (1934).

Chromium plating in the cold. O. MACCHIA and D. RAFFAELLI. *Ind. meccanica* **15,** 793 (1933).—A discussion of the problem of plating chromium at 30-50°C. as contrasted with that at 16-20°C. Conclusion: plating in the cold is entirely practicable and possesses the following advantages over the heated-bath method: The heating apparatus and its expense of operation are eliminated; a smaller current density may be used, in consequence of which a smaller investment in the generating plant and its operation is required; the apparatus required for ventilation on account of harmful vapors is reduced to a minimum. *C. A.* **28,** 1606 (1934). F. D. ROSSINI.

Electrodeposition of chromium. U. S. Patent No. 1,942,356. January 2, 1934. C. G. FINK and C. H. ELDRIDGE (to United Chromium Inc.).—The object to be plated is heated to the temperature of the bath before beginning the electrodeposition. This preheating serves to promote good adherence of the deposited chromium. *C. A.* **28,** 1609 (1934).

Chromium plating. U. S. patent No. 1,942,469. January 9, 1934. G. E. BARBER (to Timken-Detroit Axle Co.).—A ratio of chromic acid (CrO_3) to sulfuric acid of less than 40:1 is maintained in a bath containing trivalent chromium and hexavalent chromium in the proportion of not less than 1:10. *C. A.* **28,** 1609 (1934).

Coating for iron and steel. Vanadian patent No. 338,010. December 19, 1933. L. P. CURTIN and B. L. KLINE (to the Curtin-Howe Corp.).—Iron or steel articles to be coated are immersed in a bath of ferric oxalate 50 and oxalic acid 25 parts per liter at a suitable temperature, allowed to remain until a fine-grained, satiny coating is obtained, then withdrawn, washed and dried. The coating forms a bond for painting, etc. *C. A.* **28,** 1654 (1934).

Electrodeposition of aluminum. U. S. patent No. 1,939,397. December 12, 1933. D. B. KEYES and S. SWANN, Jr. (to Ellis-Foster Co.).—Articles of metal such as steel to be plated with aluminum are subjected as cathode to an electric current in a solution of an organic compound of aluminum such as an aluminum complex of the Grignard type dissolved in an organic solvent such as ether. *C. A.* **28,** 1281 (1934).

Aluminum ware. German patent No. 585,728. October 10, 1933. Vereinigte Aluminium-Werke A.-G.—A coating of aluminum oxide (Al_2O_3) is produced on aluminum or aluminum alloy ware to increase its resistance to corrosion by electrolysis. The electrolyte is sprayed onto the ware which forms the anode and the spraying pistol is connected to the cathode. *C. A.* **28,** 1282 (1934).

Coating aluminum. U. S. patent No. 1,939,421. December 12, 1933. M. TOSTERUD (to Aluminum Co. of America).—An aluminum surface is treated with a solution of an alkali metal stannate and an alkali metal dichromate, in order to form a gray coating. *C. A.* **28,** 1328 (1934).

Plating on aluminum. W. KRAUSE. *Mitt. Forschungs-inst. Probieramt Edelmetalle* **7,** 87 (1933).—The best method for obtaining a deposit on aluminum that adheres well is to roughen the surface first with a sand blast

and then nickel-plate it. Any other metal deposit can then be made on the nickel (cf. Work, *C. A.* **27**, 1829). *C. A.* **28**, 1606 (1934). M. H.

Coating metals. British patent No. 398,825. September 18, 1933. P. R. COURSEY, C. H. MARCUS and DUBILIER CONDENSER CO. (1925) Ltd.—Insulating-coatings are produced on aluminum, etc., by electrolytic treatment in two stages, the first at low voltage, e.g., 20-30 volts, and the second at high voltage, e.g., 750-1000 volts in a different electrolyte. Dilute sulfuric acid may be used in the first stage, and a solution of ammonium borate in the second. Apparatus is described. *C. A.* **28**, 1610 (1934).

Coating aluminum. Canadian patent No. 337,120. November 14, 1933. F. M. CLARK (to the Canadian General Electric Co. Ltd.).—Aluminum to be coated is immersed, without the application of externally applied electric energy, in a solution containing sodium silicate 2.5 to 12.0%, sodium hydroxide 2.0 to 6.0% and ammonia 1.5 to 2.5%, by weight. An adherent insulating coating is produced. *C. A.* **28**, 1654 (1934).

Protecting metals from corrosion. British patent No. 398,180. September 4, 1933. H. C. HALL.—Metals and alloys, e.g., iron, steel, aluminum (alloys), are immersed in a hot aqueous solution of phosphoric acid and one or more of the chromium compounds, chromic acid, chromates and dichromates. The treatment may be applied to non-ferrous metals and alloys that have been treated by the process of British patent No. 396,746 (see Monthly Rev. **20**, No. 7, 28 (March, 1934). E.B.S.), the original phosphate coat being reinforced by the deposition of chromium phosphate. Steel tools may be treated by the process to remove the surface hydrogen to render the cutting edge less susceptible to corrosion fatigue. *C. A.* **28**, 1326 (1934).

Preventing corrosion of metals such as iron and lead. U. S. patent No. 1,940,041. December 19, 1933. B. D. AVIS.—Sodium tetraborate, sodium salicylate and sodium nitrite are used for inhibiting corrosion of iron and lead when in contact with aqueous and aqueous-alcoholic solutions such as those of engine cooling systems. *C. A.* **28**, 1327 (1934).

The theories of corrosion. O. A. KNIGHT. *Mineral Ind.*, Penna. State Coll. **3**, No. 2, 2, 4; No. 3, 2, 4 (1933); No. 4, 3-4 (1934); cf. *C. A.* **28**, 86.—Knight reviews the following theories of corrosion: the acid, film, peroxide, direct oxygen attack, colloidal, biological and electrochemical. Soil corrosion and methods for combating atmospheric corrosion are discussed. *C. A.* **28**, 1647 (1934). ALDEN H. EMERY.

Corrosion studies on Hydronalium. P. BRENNER. *Z. Metallkunde* **25**, 252 (1933). — The behaviour of hydronalium (aluminum + 7-8% magnesium) in a 3% aqueous solution of sodium chloride and in sea-water was studied in comparison with duralumin and pure aluminum (99.5% Al), by inspection of the surface and by measurement of tensile strength and elongation upon progressive corrosion. The tensile strength and elongation of hydronalium in both the hard and soft states are only very slightly affected by corrosive attack by the 3% sodium chloride solution in a period of six months, whereas both decrease markedly for duralumin and pure aluminum; similar results were found for corrosion in sea-water. Although the present price of hydronalium is high, the superior corrosion resistance and the ease of welding promise wide application. Additional data are given in a discussion. *C. A.* **28**, 1647 (1934). R. F. MEHL.

Hidden sources of defects in degreasing of materials. R. JUSTH. *Oberflächentech* **10,** 271 (1933).—Unsatisfactory results in plating are often due to the degreasing process applied immediately before plating. Passivation of the surface produced in degreasing should be removed by rinsing for 1 minute in 5% hydrochloric acid, or 2-5% sulfuric acid, and subsequently rinsing in clean water. Brittle deposits might be due to electrolytic degreasing which had been carried out too long or at too high current densities; this is particularly the case for nickel-chromium plating. Electrolytic degreasing should be done for 1.5 minutes maximum, at 6-8 volts maximum. In most degreasing processes it is advisable that the pieces should not be dried after degreasing, and should be plated as soon as possible. An acid treatment of cold-deformed parts before plating is recommended to obtain an absolutely clean metallic surface on which the deposit adheres firmly. Treatment by various solutions is described. *C. A.* **28,** 1279 (1934).

Cleaning precious metals such as silver. U. S. patent No. 1,941,040. December 26, 1933. C. MANN.—A bath of sodium bicarbonate ($NaHCO_3$) solution is used containing a zinc electrode and a catalyst of activated carbon. *C. A.* **28,** 1283 (1934).

Pickling metals. French patent No. 755,107. November 20, 1933. Vereinigte Stahlwerke A.-G.—Sulfuric acid which has been used for refining petroleum hydrocarbons is diluted with water to 35.40° Baumé and heated to 90°C. for 30 minutes to one hour, or is heated to the same temperature and for the same time to the same concentration by the introduction of steam and is then used for pickling iron and steel. *C. A.* **28,** 1319 (1934).

Pickling metals. French patent No. 755,281. November 22, 1933. Imperial Chemical Industries, Ltd.—A quaternary ammonium compound, e.g., cetyl pyridinium bromide, is added to the pickling acid to reduce the attack of the acid on the metal without preventing the dissolving of the rust. *C. A.* **28,** 1319 (1934).

Pickling stainless steel. U. S. patent No. 1,939,241. December 12, 1933. M. TAYLOR (to Merrimac Chemical Co.).—A bath is used containing acetic acid 40-50, nitric acid 10-15 and hydrochloric acid 4-8%. *C. A.* **28,** 1319 (1934).

Preventing cleaned ferrous metals from rusting. U. S. patent No. 1,938,961. December 12, 1933. J. H. GRAVELL (to American Chemical Paint Co.).—The cleaned surface is coated with a film of arsenic by treating it with sodium arsenite solution. *C. A.* **28,** 1328 (1934).

The application of X-rays in the field of electrodeposition. N. PROMISEL. *Metal Ind.* (London) **43,** 437 (1933).—A review of recent work on electrodeposition, based on X-ray studies, and a summary of problems of practical importance that may be solved by this method. *C. A.* **28,** 1279 (1934).

Metal plating. German patent No. 585,708. October 7, 1933. R. KOPF.—In plating chromium or chromium alloys, acid baths of pH value below 2.5 are used. Thus, stainless steel is given a plating of nickel by electrolysis in a bath of nickel sulfate, sodium sulfate, boric acid, ammonium chloride and sulfuric acid of pH value below 2.5. *C. A.* **28,** 1281 (1934).

Metal coatings on chromium. French patent No. 754,483. November 8, 1933. Württembergische Metallwarenfabrik.—To obtain a metal

coating e.g., of copper, on chromium or an alloy thereof, such as stainless steel, an abnormally acid bath is used, e.g., one having a pH value below 2.5.
C. A. **28,** 1282 (1934).

Electrodeposition of Indium. U. S. patent No. 1,935,630. November 21, 1933. D. GRAY (to Oneida Community, Ltd.).—The article to be plated is made the cathode and an anode of indium is immersed in a bath such as may be formed of water 1 gallon, potassium cyanide 15 ozs. and glycine 7 ozs. (or cyanuric acid, abietic acid, adipic acid, acetyl-meta-aminobenzoic acid, acetyl-para-aminobenzoic acid, acetaldehyde, sodium bisulfite or meta-aminobenzoic acid, serving to prevent precipitation of the indium). A c.d. of about 0.035 amp./sq.inch or more is used to effect the deposition of indium.
C. A. **28,** 715 (1934).

Etching procedure for the metallographic examination of the alloys of the platinum metals. F. BECK. *Metallwirtschaft* **12,** 636 (1933).—Ordinary etching solutions and fused salts mixtures are not satisfactory for rhenium-platinum, rhenium-rhodium and rhenium-iridium alloys. Good results are obtained by pouring molten sodium chloride into a small graphite crucible containing a platinum triangle which serves as cathode. The sodium chloride is kept molten at about 860° C. by means of a gas burner, and the specimen to be etched, which is attached to a platinum wire, is immersed in the crucible and forms the anode. A current of 3amps./sq.cm. at 2.5volts is passed through the bath for 5 to 45 seconds, depending on the composition of the alloy. The specimen is then dipped in distilled water, dried in alcohol, and is ready for examination. *C. A.* **28** 1005 (1934). C. E. MacFARLANE

Composition for cleaning articles of aluminum, zinc and tin. U. S. patent No. 1,935,834. November 21, 1933. F. DRAISBACH (to Chemische Fabrik Joh. A. Benckiser G.m.b.H.) Tri-sodium phosphate is mixed with 15-20% of aluminum sulfate to prevent corrosion of the metal by the phosphate.
C. A. **28,** 869 (1934).

Some new detergents. A. S. RICHARDSON. *Oil add Soap* **11,** 10 (1934).—One class of new detergents consists of alkyl sulfates, e.g., sodium lauryl sulfate. To date they have been sold mainly in the textile industry; and their most interesting property is resistance to precipitation in hard water. Another class of new cleansing agents may be called acylated sulfonates, one type of which consists of the fatty acid esters of hydroxy-sulfonates of low molecular weight. Another type consists of the corresponding acid amides in which the oxygen involved in the ester linkage is replaced by NH or NR. These compounds have cleansing and sudsing power similar to that of soap or alkyl sulfates. The molecular formula is capable of variations similar to those of the aklyl sulfates.
C. A. **28,** 1211 (1934). E. SCHERUBEL.

Soldering aluminum bronze. French patent No. 751,370. September 2, 1933. P. ODAM.—A cleaning agent which permits aluminum bronze to be tinned and soldered is composed of a hydracid and a volatile base such as ammonia, hydrazine, methylamine, or ethylamine, in solution in phosphoric acid.
C. A. **28,** 1014 (1934).

IN THE UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF CONNECTICUT

UNITED CHROMIUM, INCORPORATED, Plaintiff

vs.

GENERAL MOTORS CORPORATION, THE NEW DEPARTURE MANUFACTURING COMPANY, THE BASSICK COMPANY,

Defendants.

In Equity No. 2284

In the U. S. District Court
Hartford, Conn.
Thursday, 15th March 1934
at 10 o'clock a. m.

ARGUMENTS ON FINAL HEARING

Before:

Hon. EDWIN S. THOMAS,
U. S. District Judge

Appearances:

Messrs. GIFFORD, SCULL & BURGESS, (New York City) by
GEORGE F. SCULL, Esq.
of Counsel for Plaintiff

Messrs. COOPER, KERR & DUNHAM (New York City) by
DRURY W. COOPER, Esq.,
FRANK E. LIVERANCE, Esq., (Grand Rapids, Mich.)
of Counsel for General Motors Corp., and The New Departure Manufacturing Company.

Messrs. FISH, RICHARDSON & NEAVE (New York City) by
MERRELL E. CLARK, Esq.
HAROLD F. WATSON, Esq.
of Counsel for the Bassick Company.

DISCUSSION

The Court: I suppose you are ready with this Chromium case.

Mr. Scull: Yes, your Honor. I do not know how you want the time divided. We were going to dispose of it today if we could, and how shall we divide the time?

The Court: I have plenty of time. You do not need to be curtailing time on my account. It is entirely up to you, gentlemen. If you do not finish today, we can go over until tomorrow. If you want to finish today, it is satisfactory to me.

Mr. Scull: I would rather try to finish today. We will assume that you will sit until 5 o'clock.

The Court: I will sit until 5 if you finish today. If you do not finish today, I would like to sit until half past four. In a case of this kind I do not like to have you feel that you have to hurdle over some points that you want to touch upon and thereby get within the time limit. On the other hand, I would assume that both sides will confine yourselves to those things regarded as essential, and not bother with the non-essentials, and if you want to finish today, I will sit until 5 o'clock.

On the other hand, it is 10 o'clock now and you may feel differently yourselves along about 3 o'clock. It is rather flexible, Mr. Scull, and I do not feel disposed to hold you or the other side to any one minute and put the gavel down and say "You are through."

So that as far as the division of the time is concerned, of course if you argue today and quit at 5, each side would have three hours. Of course, if you want to stipulate to bind yourselves to three hours, it is satisfactory to me. What have you to say, Mr. Cooper?

Mr. Cooper: It is agreeable to us, your Honor. I have always found that in the oral presentation of these cases that have been thoroughly briefed, the

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utmost counsel can hope to do is to elucidate the main points and get your Honor familiar with the respective positions throughout the case.

More than that, no human mind could take in all the niceties of statement or argument, and it would seem to me that a division of substantially three hours a side and completion of the thing today would be proper on both sides.

The Court: All right. Then we will understand that we will adjourn sharply at one, and come in sharply at two, and adjourn sharply at five. That will be three hours on each side.

OPENING ARGUMENT ON BEHALF OF THE PLAINTIFF

Mr. George F. Scull: I am handing your Honor a copy of the patent in suit and a copy of your own Opinion in the *International* case, and a copy of the Opinion of the Court of Appeals in the *International* case.

As your Honor knows, this patent, which has already been in this Court in the *International* suit, was held valid and infringed, and this Court's decree was affirmed by the Court of Appeals.

In this suit the defendants are three, the New Departure, which has a place of business at Bristol; The Bassick Company, which has a place of business at Bridgeport. These are direct infringers, we say.

General Motors admittedly owns all of the common stock of the New Departure Company and some of the preferred, as I recall, and particularly admits that it controls and directs the policies of that company in business. It admits that the place of business of New Departure is also its place of business, that is, General Motors admits it has a place of business in this State.

Consequently, we say that General Motors is directly before this Court by reason of the acts of its subsidiary, its *alter ego*, the New Departure Company.

There have been two cases decided where General Motors has tried to escape the jurisdiction of the Court under circumstances exactly the same as they are here, and both of them have been decided adversely to General Motors. One of them was decided by Judge Killits, at the time we had written our main brief. In the other decision which I did not see until after we had written our main brief, I find that Judge Lindley in Illinois had held to the same effect, and in the case of Judge Lindley he held that the acts of these subsidiaries were so much the acts of the General Motors that he actually held General Motors in contempt because the subsidiaries had disobeyed an injunction.

In Judge Lindley's case it was necessary for him to reach what he called a proper inference from the facts before him that General Motors controls and dictates the policies of such subsidiaries.

In this particular case that is specifically admitted in the Answer; and so I think your Honor will have no difficulty in seeing that General Motors is here in this District charged with infringement by the acts of its subsidiary, New Departure.

In addition General Motors is also here as a contributory infringer, bearing in mind that it has a regular and established place of business in the New Departure plant. It has committed infringing acts because the infringement by the Bassick Company is by way of a licensee of General Motors. General Motors sent its agent into this district and initiated the first bath at the Bassick Company, installed it and directed the operator there how to maintain it and carry on the process, and furthermore, from time to time, took samples of the bath for purposes of analysis, so that we say that General Motors is doubly an infringer here, first, directly through the New Departure, and secondly, contributorily through the Bassick Company.

As far as the patent in suit is concerned, I do not know that I need to say very much. I do not know how much your Honor may remember from your decision of several years ago, but I imagine from the care that you put on that decision that you do remember a great deal about it.

The Fink patent in suit is for a process, a process which Fink says, and this Court has found, was to produce a practical and commercially available process for chromium plating, something that had not been known before.

It was to be something which could be maintained continuously, that is to say, once having started the bath in operation, means are provided in the

process by which the bath could be maintained in operating condition continuously.

Chromium plating *per se* is not new. That was recognized right in the preamble of the Fink patent. But as I say, what the Fink patent discloses is a process which will give this sure and certain method of operation by which chromium plating can be done continuously.

In this Court's Opinion in the *International* case, five rules were enumerated, and so far as I know they are still perfectly good rules. There has not been anything in this case which disturbed what this Court found at that time.

First, that in the preparation of the electrolyte, all the stable radicals, for example, SO_4 must be computed whether originally in the chromic acid in the catalytic agent or otherwise, entering the bath, and then the amount is determined.

Third: The quantity of the catalytic agent should be regulated within set limits for continuous operation.

Fourth: By adding or subtracting from the quantity of catalytic agent the stable radicals already present in the chromic acid solution, the necessary amount to bring the total amount up to or down to the given limits.

Finally, the temperature ranges are given, 15 degrees C. to 40 degrees C. with a corresponding variation of the current density of from 1/4 to 1 ampere per square inch.

So far as the patent itself is concerned, the Court of Appeals has a paragraph in its Opinion starting off that the disclosure is unusually clear and it describes the invention pretty much as your Honor did in those five rules.

I shall not take time to read that.

So far as I know there has been nothing said here to affect anything which either Court said as to the disclosure of the patent.

The first defense which is raised here is an attack on the patent because of a statement in it which reads "substances I find available"—and this is on page 1, beginning at lines 52—

> "substances which I find available as catalytic agents and which remain stable, are acids and bases having a sulphate radical; acids and bases having a fluoride radical; acids and bases which have a phosphate radical; and acids and bases which have a borate radical."

The attack is based on the proofs that the phosphate and borate radicals are weak catalysts. Professor Stevenson said that they do have a catalytic effect. He said that they could not be used alone, but that their effect must be taken into consideration.

The question is: assuming (which is not the fact) that Fink was mistaken when he said that the phosphate and borate were catalytic agents, does it make any difference in law? I say it does not.

You must bear in mind that the Fink process or Fink invention is a process and in enumerating the catalytic agents which might be used, Fink would have satisfied the law if he had mentioned only the sulphate, as to which, of course, there is no question. He would have satisfied the law if he had mentioned merely the sulphate and the fluoride, as to the efficiency of both of which there is no question.

Therefore, when he mentioned two others, which let us say for the purposes of this argument, have no effect whatever and that he was mistaken as to that, does it invalidate his patent?

There is one decision which we have in our brief in which the Court of Appeals of the Sixth Circuit said:

> "We do not overlook that the specification sets out an additional particular method and advantage which were perhaps impossible of attainment"—this is at page 4 of our reply brief—" but that would not prevent due protection for the result which was accomplished."

But the fact is, as I say, that Fink was not mistaken when he mentioned them as catalytic agents. You must bear in mind that Fink was interested in the total catalytic effect of all the catalyst radicals that might be in the bath, something, by the way, which no one, so far as I know, in this case, and certainly not in the other case, has suggested was old.

Fink, you will remember, in his directions said that the amount of the catalytic agent or agents, stable radicals, means the total of such agents whether of one kind or another, that are actually in the bath. That is, all the stable radicals must be computed whether of one kind or another.

Consequently, since according to the proofs here the borate and the phosphate do have some catalytic effect, it is obvious under the Fink directions that their effect must be taken into consideration in connection with the total effect of the radicals present.

When Fink, therefore, called attention to that, he was not saying, as our friends say he said, that they could be used alone. What he said was that they are agents which must be taken into consideration.

But our friends say that the patent is invalid particularly under the doctrine of the *Corona* case. In the *Corona* case the invention was the use of D.P.G., which is a short name for a chemical substance used, as an accelerator in vulcanizing rubber. That is all there was to the invention. The Corona patentee had a claim which called for all kinds of guanidins rather than any specific kind, as accelerators, and the proofs there showed that there were some guanidins which did not function, and the Court said that his claim was too broad, broader than his invention, it covered things which did not work.

In this particular case, the case at bar here, we have not any claims for any particular radical. Our claims are for a process in which the radical is simply one step or one element, if you will, to be used in that process. Consequently, it is not the same thing as the *Corona* case by any means, and has no relation as I see it, to the doctrine in that case.

The second point of defendants, is not really an attack, it is part of the game which our friends are playing with the patent. They are trying to crowd the Court to the position of saying that the Fink invention consisted in ascertaining that you did not have to use chromium sulphate and that you could use the SO_4, and so they say that, so far as the fluoride is concerned, Fink has no claim on that because he got into an Interference with one Hambeuchen. The claim in Interference was for a "bath for electro depositing chromium containing chromic acid in excess, and a soluble metal fluoride, and being substantially free from acids other than chromic," and in that Interference Fink took no testimony and automatically the patent was awarded to Hambeuchen.

From that our friends say that Fink cannot claim the fluoride. According to our friends, he can not claim the sulphate either, because the sulphate was used in chromium plating baths before Fink. Sargent, for instance disclosed the use of chromium sulphate, so that if you follow our friend's reasoning, you would also be obliged to exclude the sulphate from the Fink patent and there would not be any catalyst left.

They say the borate and phosphate do not work, the fluoride is out because of what occurred in this Interference, and the sulphate of course must be out, because that was known before. Ergo: There is nothing left to the Fink patent. They do not go quite so far as that latter, but that is what their logic would lead them to. The fact is that this Interference with Hambeuchen had no relation to the Fink patent here in suit. In the first place, it called for a metal fluoride. Hydrofluoric acid which would contain the same acid radical as the metal fluoride. Hambeuchen said specifically that hydrofluoric acid is not the equivalent as a metal fluoride.

In the Fink patent in suit, the emphasis of the whole thing turns on the acid radical regardless of what it is connected to.

So that you can see that in that particular case, Fink might very well not be able to assert claims as to a bath made up with a soluble metal fluoride, and yet it would have absolutely nothing to do with his use of the fluoride radical in his process.

Again, while I am on the subject of the patent, although I do not think that they make any particular point of it so far as the patent itself is concerned, they do in connection with some of the defenses, and that is as to whether or not the critical limits which Fink gives, that is, the ratio of 250 to 1 to 50 to 1 are of any importance. Our friends assert that they are not.

They base all that on some testimony that was taken out West last summer as to the procedure followed by the Timken Detroit company. There, they are doing a certain kind of plating and have evolved a certain process. I do not know what it was that inspired them to do it, but I think it was an attempt to avoid the Fink patent. In this process they have gotten the ratio up to or down to, which ever you want to call it, something like 20 to 30 to 1 instead of 50 to 1, and they are doing plating, not ornamental plating, but this heavy plating, depositing metal on parts to build them up.

Our friends say that under certain conditions the Fink ratio is of no importance. Let us see what those certain conditions are.

At Timken, which is the particular place they are talking about (it is the one place in the country apparently that they could find where that thing is being done,) the whole situation is so delicate that they had to space the anodes and the cathodes from 1/4 to 1/2 an inch apart, and their own people admit that a variation of 1/32 of an inch in that spacing will cause non-uniformity, and that as much as 1/8 of an inch would probably produce no plate at all.

In other words, they have got things just on the ragged edge so that the very slightest variation even for the particular kind of plating they are doing, would throw them off. Furthermore, instead of a current of about 150 amperes per square foot, they are using about 900 amperes per square foot. In other words, they have evolved certain special conditions for their very special kind of plating which they are willing and content to go along with, and that is the reason our friends say that the critical limits set up in the Fink patent are of no importance.

Well, the answer to that is that for some reason or other, the defendants themselves are very careful to operate their baths at substantially Fink's optimum of 100 to 1. Phillips, who is the head man of the General Motors in their plating operations, said as late as 1929, the total content of sulphate should be 1 per cent. of the content of the CrO_3, that is, $\frac{100}{1}$, which is Fink's optimum. So that it may be that Fink did not foresee all of the possibilities of changing conditions so that it might be possible to operate outside of the limits which he specified. But nobody is interested so far as this case is concerned at any rate, in those very special conditions.

We have shown in our brief the circumstances under which Fink made the invention here. The importance of it is that we say that Fink's date of invention is the middle of May 1924. I shall not rehearse the proofs. I think your Honor has heard them. Fink, you will recall, is a Professor at Columbia. Out of his probably meagre salary he employed two or three assistants and because of his interest in chromium plating, he set up a bath there and began investigations, paying these men out of his own pocket. They finally got into difficulties in their experimentation because of the different results that they got from different lots of chromic acid which caused them to have those lots of acid analyzed, and they found there, and then, that the difference in the results was due to the difference in the amount of the SO_4 radical in those different chromic acids. From that and other investigations Fink made his discovery that it was the acid radical which was the determining factor.

He had a bath at that time called No. 60. He began operating it in accordance with his invention and he continued operating it down at least to the last trial, and I think the bath is still in existence today in his laboratory.

Our friends say that there is no evidence that Fink ever had any idea of what the chemical make-up of his bath was. That I do not think is quite true. At page 18 of our reply brief, I have reproduced one of our exhibits which shows what he did know even as early as May 1924. There he had determined what the SO_4 content was in the several chromic acids. When he tried them under varying conditions, he found, for instance, that when the ratio was 312, it would not work satisfactorily, and you could not get bright plate. When, however, the ratio was 156, he found he "got plating on copper which was fine; a ratio of 138 throws very well on copper, plate on copper is bright." At a ratio of 89 he gets bright plate on copper. At a ratio of 12, however, it would not work except at high C.D.S. due to high sulphates. Those are all quotations from the cards which he wrote at that time.

So he did know within reasonable limits what the composition of his bath was chemically.

But now came a thing which our friends do not seem to comprehend. Fink did not rely on chemical analyses of his baths for their maintenance, but he evolved what to my mind is a much more beautiful way of maintaining his bath, one which certainly is more certain and certainly more simple than chemical analysis and it is what he called "physical analyses."

The point is this: He observed the physical phenomenon which occurred for different compositions of the bath when those baths were operated under standard conditions. He found, for instance, that if the catalyst was present in certain amounts, the hydrogen would evolve continuously, whereas in other amounts it would evolve intermittently. He also observed the result of plating on these standard plating strips.

For instance, this is illustrative only, the testimony of all his witnesses is that their practice in maintaining their baths was to use one of these standard strips of brass, which was put into the bath and they knew from experience and from observation and checking against baths which had the known amount of catalyst in them, the results which would appear on these strips.

Well, consequently knowing that, if I want to test any particular bath, I drop my strip in and I plate it under standard conditions for a standard length of time, and then I pull it out. Now, then, this being the known variation according to the amount of sulphate, of course I can immediately determine what is the condition of my bath by comparison with this strip. For instance, if the bath was under-sulphated, you get just a brown scum and no plating practically at all. As the sulphate increases, the ratio goes down, you commence to get an irridescent, mottled appearance, and as you keep on increasing the sulphate to bring it more towards the Fink limits, you commence to get chromium plating but not clearly defined until finally you commence to get a perfect plate with the top edge, that is to say, the line which corresponds to the level of the liquid in the baths, is a straight line.

If you keep on adding your sulphate, by and by you commence to see that instead of getting a straight line across the top, you get this meniscus. I do not know whether your Honor can see it or not, we talked a lot about it at the trial, but you can see at the middle section, (I am calling attention to Exhibit A-2), the line is substantially straight, but as you get along it commences to get a little dent in it, until by and by you get a well-defined meniscus, as it is called, that is a crescent-shaped curve. Finally, if you get too much sulphate, you get no plating whatever.

Actually, these particular strips that I am talking about were made in the early part of 1925, for another purpose, but they illustrate one part of what Fink used for his physical analyses.

He maintained his bath, as I said, by means of that. There is a double advantage. In the first place, that test could be made in half an hour, whereas the proofs here all show that a proper analysis for SO_4 for example takes at least 24 hours.

Secondly, by this means he was determining the total catalytic effect of the acid radicals that might be in his bath. He did not have to know definitely whether there was any nitrate or any chloride present. All he was concerned with was what was the total catalytic effect. If it was not sufficient, he could correct it by putting in some more sulphuric acid to increase the SO_4 part, but he could always bring up his total catalytic effect to the desired amount, without the necessity of knowing specifically how many radicals were there, or in what quantities.

If he had analyzed only for the SO_4, as some of the alleged prior users did, he could easily be thrown off because the analysis for SO_4 would not have shown him what other acid radicals were there.

And so by this, what I say, beautiful idea, he was able to carry out this plating to maintain the bath No. 60 continuously as long as he pleased, in fact, down to the present time.

By the way, I may say this on that question of physical analysis. It was rather interesting to me. In connection with another matter, I was cross-

examining Professor Willard, and there was some statement that there in one of Mr. Phillips' writings he had referred to an irridescent appearance on a strip, or on some plating, and Professor Willard's answer indicated that he associated the irridescent appearance with the low sulphate in the bath, showing that even today the users of chromium plating do, wittingly or unwittingly, follow this physical analysis which Fink knew so well.

Burr left Fink in 1927 and went to one of the Bank Note Companies, and he used physical analysis there to maintain his baths.

So that there can be no question that this physical analysis which Fink used, is a perfectly good and proper method of maintaining the baths, and when our friends say that he could not have had the invention until he made his first chemical analysis, they are simply talking nonsense, because of course he was operating clearly in accordance with his invention from the time he began in May 1924 with his appreciation of the effect of the radical.

I do not know that I need speak very much on Fink's commercial success. Your Honor may recall that he began plating for pay at Columbia in 1924 and then the Chemical Treatment Company took an interest in the thing and set up a plant at Center Street, when toward the last of 1925, and the beginning of 1926, the large plant which is down here at Waterbury was opened, and that has been running continuously.

There have been and are still hundreds of licensees under the Fink patent, and this Court and the Court of Appeals both found that it was Fink's contribution which started the art on its way.

In connection with our Waterbury plant and our licensees, our friends have taken a great deal of testimony to show the troubles which we had. I am not going to spend very much time on that, because if I did we would be here a long time. But they have combed the country to find all the "sore head" ex-licensees usually who in their attempt to injure us, have gone on the stand to complain about the troubles which they had in starting up. It would be very phenomenal indeed if any new invention could be applied to all sorts of conditions, as was attempted in the case of this chromium plating, without having some initial difficulties. But the curious thing is, so far as I recall the evidence, in no case was there ever any complaint that the bath was wrong. That of course is the Fink invention. Most of the complaints came about because of what is called "peeling." When you trace that back, you will find that that in turn was due to the peeling of nickel from the body of the article and not the peeling of chromium from the nickel.

I may just take a minute to say that there has developed and has been in use for some time, (I shall not go into the reasons for it) the method by which if you want to chromium plate an article you first nickel plate it. In fact, in some cases they actually copper plate first and then nickel plate and then chromium plate over that.

Farr of Manning-Bowman, who was anything but friendly to us, when he was called as defendants' witness, was very frank to say that all the troubles of this peeling came about because nickel plate before chromium plating, had fallen into rather dubious ways, that people were getting away with murder so far as nickel plating was concerned, and I presume that that is one of the reasons why nickel plated articles in your house and in my house, go bad as quickly as they do. But when they attempted to put chromium on top of this badly deposited nickel, the thing was shown up at once, and it began to peel, and they had to come back and apply to their nickel plating what they should have done in the beginning.

That is to say, if the nickel plating is done properly, then chromium plating gives no trouble. For instance, one of the watch companies here, have always done proper nickel plating and consequently when they wanted to put chrome on top of their nickel, they could do it without the slightest trouble. They never had the slightest trouble.

Then defendants brought our ex-employee Crowder from the South, to tell all the troubles which he had at Waterbury when he was put up there in charge of the baths for the Chromium Corporation, or rather the Chemical Treatment Company, when it first began. Cross-examination brought out that prac-

tically all of his troubles which he complained of, were due to what I think should have been avoided if ordinary common sense had been used. For instance, one case struck me particularly. He said they had some failures because the temperature of the bath was not maintained. I asked him did he not know at what temperature it should be maintained? Yes, but the workmen let it drop. That is one of the troubles in employing the Fink invention about which they complain.

Then they had trouble in plating certain articles. For instance, somebody evolved the idea that, if you plate tire moulds with chromium, you would get a hard surface, and so they sent over a lot of their old tire moulds which were all pitted, with the old rubber, and what-not, in those holes, and they tried to chromium plate over that dirty surface and they had trouble. Of course they did. Because if there was any one thing that the cross-examination brought out, it was the most elementary thing that in any kind of plating you must have a clean surface.

I shall not go into that sort of thing, but our friends have finally gotten around to asserting for one reason or another, that that has some bearing on this case. I insist it has not. So much for the Fink patent.

Now, the defenses. In spite of the very thorough consideration which the Sargent article had before your Honor, and in the Court of Appeals, and the rather effective way in which both Courts disposed of it, we find it being used here again as a primary defense. I do not know that our friends go quite so far as to say that they want this Court and the Court of Appeals to reverse themselves as to what they found as to the Sargent article, but they are using it at least to justify what they are doing, and they even have a chapter in which in their heading they refer to the dependability of Sargent.

If there is any one thing I think which this Court found in the other case, and the Court of Appeals affirmed, it was that Sargent was not dependable.

Your Honor may recall that in the other case, there were, I think, four alleged prior users, one after the other of whom were shown to have attempted to follow Sargent, and did not get anywhere; and yet, in spite of that, and a great deal of that testimony is reproduced here by stipulation, in spite of that we have our friends asserting that Sargent after all is dependable.

This Court, for instance, in speaking of Sargent under a heading or in a paragraph that referred to all of the publications that had been put in, said:

> "These publications can not be permitted to anticipate because *inter alia* the proportion of SO_4 or other catalytic radicals contained in the chromic acid is not set forth."

That is the key to the situation so far as Sargent is concerned. Sargent said that he used C.P. acid. The proofs in the *International* case, and they are reproduced here, show that at his time, C. P. could not mean anything. I was rather surprised personally, and I think perhaps the Court would be, to find that "C.P." is a meaningless term so far as purity is concerned. It is simply a mark which any manufacturer of chemical, and particularly of chromic acid at that time, put on his material without regard to what it really contained, and the proofs show that C.P. acid was on the market that had as much as 3 per cent. sulphate in it, and in one case I think it ran as high as 12 per cent. That seems rather extravagant, but that is what the proofs are.

At any rate, one of the things that Sargent did not tell his readers was that the chromic acid should be free from radical. There were a number of reasons why he did not. In the first place, he had no appreciation that the radical alone counted, and so of course he could not have told them that. No one could read his paper and get a correct process to follow in doing chromium plating.

The proof of that is that nobody did.

I might just read you here what the Court of Appeals said about Sargent:

> "Sargent did not intimate that the radical alone need be controlled. He had not found the critical element and his followers never could depend upon his process."

Yet, as I say, we have our friends here now saying to this Court that Sargent is entirely dependable.

I think that needs explanation on somebody's part.

In that connection I want to refer to what happened to General Motors just briefly. They also were followers of Sargent. They began in a laboratory way sometime as I recall in 1924, and they did a great deal of work, and I shall not go further than to refer to what appears in the record as a grand fiasco, due to the fact that Sargent is anything but dependable.

Mr. Phillips wrote an article, page 1369, in August 1926, in which he said:

> "After some years working on a plating process"—(and by the way, I put him on the stand to have him agree that that was a chromium plating process)—"we were attempting to put it into production and it started off beautifully, so much so that we decided on a much larger unit. Improved equipment was ordered and installed, solutions carefully made up and finally the day arrived to try it out. Officials of the organization were present and the work put into the tank as usual. All the motions were gone through in apparently fine shape. When the work was taken out, nothing had happened. It had no more plate on it than it had when it went in. It took two months of hard work to find out that one of our chemicals had contained impurities which made the operation of this bath impossible."

That incident as I recall, occurred sometime in 1925, the latter part of 1925. I say that it throws a great light on our friends' statement now that Sargent was dependable. Here is General Motors after a great deal, he said several years, experimentation start out to do commercial chromium plating and that is what happened.

While I am on the subject of General Motors, I think it is interesting to call attention to this fact. Their contemporaneous writings when they were doing this experimental work, show that they were speculating on how long the bath could last. It has some bearing in this respect, that the question will arise:—is the continuous regulation of a chromium plating bath anything that those familiar with plating of all sorts would not know all about? Certainly it did not appeal to the General Motors people that it was an obvious thing that a bath could be maintained indefinitely, because they actually first proposed to make life tests, as they called them, and they made those tests and from those tests they estimated that the life of the chromium plating bath would be three months.

Actually when they got into practice and began using the Fink invention, their baths ran on forever and ever, as their own proofs show.

In connection with Sargent, defendants also assert that the experiences of Udy, (I shall come to that in a minute); Manhattan, (I shall come to that in a minute); Westinghouse, Eastman, and themselves showed that Sargent is dependable. The Westinghouse defense is reproduced here by stipulation precisely as it was in the *International* case. Both this Court and the Court of Appeals rejected it as an anticipation. The reason they rejected it was as stated by the Court of Appeals, that although it was shown that at times in their endeavor to follow Sargent, the Westinghouse Company occasionally happened to have baths that came within the Fink limits, nevertheless as late as December 1925, the Court goes on to say "occurred baths in which both the chromic sulphate and the radical ran well above his maximum. The proof is too uncertain that they had even established any limits to the ratio in terms of the sulphate; it certainly does not disclose the idea that the radical alone was important."

In other words, if and when Westinghouse got any satisfactory plating, it was purely accidental, and they could not reproduce it.

Your Honor knows very well what the law is on that subject. So that when our friends now say, in spite of the findings of this Court and the Court of Appeals, that the Westinghouse practice or the Westinghouse prior use shows that Sargent was dependable, they are just contradicting, in my opinion, not only the findings of this Court, but what the proofs themselves show.

They also refer to Eastman. The proofs as to Eastman are very much like those of Westinghouse. I have forgotten the number of different baths Eastman experimented with. They could never duplicate their results. It is the same story exactly as Westinghouse. They got good results sometimes

if they happened to have the right composition, but they could not reproduce their results because they did not have the key to the situation.

As far as defendants are concerned they point to their own alleged following of Sargent. They are surely getting certainty in their results. They are doing chromium plating continuously from day to day, and they are getting beautiful results. No question about that. They point to that and say that they are following Sargent, and therefore that proves that Sargent is reliable.

I think the Courts have learned long ago to be very suspicious of defendants who come in after the patent in suit and claim that they are using successfully the process or disclosure of a prior art publication, when prior to the patent in suit nobody was able to do it.

I think it excites a rather strong suspicion that the intelligence which our friends say that they are using in applying Sargent, is due to something more than just gray matter; that probably it is due to the patent here in suit, to wit, the Fink invention.

In connection with Sargent, I just want to touch for a minute on another phase which crops up repeatedly in our friends argument. They keep on saying and talking about the Sargent formula as if it were something ready and waiting for anybody to go out and use. If there is any one thing which the record in this case and in the *International* case showed, it was that there was no such thing as the Sargent formula; that that is merely a label that our friends are applying to a particular quantity of chromic acid and chromium sulphate. But so far as any formula by which anybody could produce continuous chromium plating, it does not occur in Sargent. It is not to be found there.

And again, they even say that Sargent's ratio figures out to be 1 to 111. There again they are doing exactly what the defendant in the *International* case did. The Court of Appeals said:

> "The defendant repeatedly speaks of the ratio between their"—in that particular case it was the Bureau of Standards—"chromic acid and their radical. They knew no radical in the sense that the art knows it."

The same thing applies to Sargent. He knew no radical, he could not have had any ratio because he did not have the very essential thing necessary before he could reach such a thing as a ratio.

One of the new defenses which we have here is the socalled Udy defense.

Udy was an electro-chemical research man employed by the Union Carbide and Carbon Research Laboratory at Niagara Falls. He began there his work in 1922 primarily to develop a means by which chromium metal could be taken from chromium salts. Your Honor may recall that. Here is a little bottle showing the nodules such as he produced, and he himself has some samples showing what he was primarily driving at, that is to say, to cause the chromium metal to be deposited in thick masses on a cathode and then be broken off to be used as pure metal.

This Udy defense comes into this case with a rather unusual status. I want to say preliminarily, or I want to explain preliminarily just what the situation is. Udy filed an application for his so-called process in June of 1924. That application did not contain much which is in the Fink patent. Udy went on prosecuting it in the normal way until the issue of the Fink patent. Immediately Udy copied the claims of the Fink patent and amended his specification in a number of ways which we think are improper, but which the office permitted, and an Interference was declared.

That Interference was going along until in 1927 there was a consolidation of interests between the owners of the Udy application and the Fink patent. Each side owned not only these particular things but a great many other patents and applications relating to chromium plating, and there was a consolidation, and as a result, the ownership of the Udy application and of the Fink patent came into the plaintiff's hands. As I say, the Interference was pending.

The matter was referred to Mr. Gifford who had been the attorney for the owners of the Fink patent and to one C. P. Townsend who was head of the Patent Department at that time of Union Carbide, and who was familiar with the work of that company.

These two attorneys decided that a concession of priority should be filed by Udy and consequently the Udy application became abandoned.

I shall have something more to say about all this later, but I want your Honor to understand before I begin to talk about the Udy defense, that the situation is one which the Court of Appeals, our Court of Appeals, spoke of as a "technical abstraction." At the bottom of page 51 of our main brief, I quote from *United Shirt vs. Beattie*. The Court said:

> "The question is at best a technical abstraction. No rights of rival inventors are involved, as the complainant, the Shirt & Collar Company, was assignee of both Pine and Dormandy, and that company with full knowledge of the facts, took the patent, in accordance with what appeared to them to be the truth, in the name of Pine. A decision against Pine now will benefit infringers but will be of no benefit to Dormandy."

Paraphrasing that, a decision now against Fink will benefit infringers, to wit, the defendants here, but will be of no benefit to Udy, Udy having transferred all of his rights long ago.

So now let us look at this "technical abstraction" and let us see what the Udy defense amounts to. First, I want to take some time on the work of Udy. Defendants divide his work into two parts. During the first part, beginning in 1922, they say he was following Sargent and getting consistently good results, both of these assertions we challenge.

That work continued down until sometime in December 1923, when they come to what they call the second phase or the second part of Udy's work when he is supposed to have made the Fink invention, and we say that he did not.

The first part is only of interest because our friends use it first, as they say, to show that Sargent really was dependable, and second, to justify their own procedure.

Let us just examine for a minute what Udy really did in that period.

First, let me state that Udy's work is to be found only in the records which he kept. There are no witnesses produced other than Udy himself. No proofs that any of his reports were ever read, particularly those on which the defendants here rely. There was no publication of any sort of anything which he did; and the fact is that all that he did was the ordinary research work in a chemical laboratory, where such work was kept secret and confidential; and as I say, most of his work was directed to this so-called electro-winning; that is to say, getting of chromium metal from the salts; for use of the metal as a pure metal, and not for plating, as we think of it in this case.

Udy did do plating on odds and ends from time to time as samples, and test pieces, as he said. Now, all of his work both at this time and later, was experimental. In true research laboratory fashion, he was trying to find out the facts; and consequently he would set up a bath and vary the conditions. He was not making continuous runs in order to produce commercially any particular thing; but he would vary his conditions from day to day, in order to find out what would be the effect of this, that and the other thing.

Very early in his work, he adopted what we have called the "barium precipitation" method in connection with his chromic acid. He found that the chromic acid which he was buying was filled with what he calls impurities. To him at that time, sulphates were merely impurities; and he adopted this barium hydroxide precipitation method in order to remove those impurities, most of which were sulphates; and he continued that method for getting what he thought was pure chromic acid throughout his work; in fact, he carried it down into his application for patent.

Now, the proofs here show that that particular method which he proposed is not a practical method.

Professor Stevenson pointed out why. You see, you take the barium hydroxide and add it to your chromic acid, and the theory is that the sulphate in there will combine with the barium, and since that is insoluble, it will be precipitated and be taken away.

Now, in a clear water solution, where sulphate is present, that seems to be a fairly good method; but in a chromic acid solution, difficulties are encoun-

tered. Professor Stevenson says, for instance:

"If we then come to the real situation and take a chromic acid solution used for replenishment * * * then it is exceedingly difficult in practical operation, to precipitate all the SO_4 from that solution, and yet not have an excess of barium. That is an exceedingly delicate matter.

The Court: What page?

Mr. Scull: That is page 1413 and page 1414 of the record.

Udy himself agreed with that, and we reproduce his testimony at page 50 of our reply brief. His first answer—I was talking in this particular case about something that he had done in the way of plating graphite strips in December, 1923, by the use of his socalled process, and I was asking him about this very use of the barium hydroxide precipitation, and Udy was quite guarded, but he had to make certain admissions, because it was the fact, when he was asked—

Q. "And it is sometimes a bit uncertain, is it not, as to whether all sulphate would be removed from the chromic acid?

A. "Yes, it is quite a delicate job to remove all of that."

That is page 50 of our reply brief.

Now there is a second element that comes in, and that is that while treatment with barium hydroxide has an effect on the SO_4 radical, it has no effect whatsoever on the other acid radicals which may be present.

At pages 1415 and 1416 of the record, Professor Stevenson calls attention to that; and Udy himself in one of his own writings, at page 204, of Exhibit No. 71, says that the barium hydroxide precipitation fails to take care of acid radicals other than SO_4.

So that this method by which he thought he was getting pure chromic acid, turns out to be a very uncertain thing. No one can tell by its use what is the actual composition of the chromic acid solution which is obtained. That has a particular bearing on what I will come to shortly, when we come to the second part of his work.

Now, in addition, throughout this period that Udy was supposedly following Sargent and getting consistently good results, in the language of our friends, curiously enough, he now admits that his analyses were "all off;" and he has to admit that, because in March 1923 he made what to him was a very wonderful discovery, a discovery which they now are obliged to say that Udy himself says was entirely erroneous.

He thought that he was chromium plating from pure chromic acid, something which concededly can not be done. He was reaching those conclusions because his analyses for sulphate were so erroneous that he thought he had no sulphate whatever, when, of course, he must have had sulphate or other acid radicals in considerable quantities, as we know now.

Now, those two things which Udy was doing in 1923, when he was supposed to be following Sargent, of course, vitiates practically everything that he did.

In the first place, of course he was not following Sargent, because Sargent certainly never taught anybody anything about using barium hydroxide as a precipitating agent for the chromic acid. In the second place, Udy was analysing his baths, something which Sargent certainly never taught should be done; so that he certainly was not following Sargent; and moreover, he was not getting consistently good results. I shall not take the time to go into details here, as it is covered in our reply brief, page 35, where we have shown that he was just floundering from one thing to another, trying to find out what was the matter with the baths with which he was working. He could not get consistent results.

For instance, I think the last thing of all that he wrote about Sargent, will dispose of the whole matter. In March 1924, after he evolved what is called the "Udy process", in his reports, he wrote this, found at the bottom of page 43 of our reply brief, and he included Sargent because he had previously referred to Sargent—

"Early investigators have investigated the electro deposition of chromium but none of them seem to have produced a process which has been commercial, or that will give uniform results over long periods of operation. This

fact is borne out by the common knowledge that at the present time the electro deposition of chromium is commercially unimportant."

Now, that was written by Udy after he had been experimenting with Sargent for more than a year, during which time our friends say that he was finding that Sargent was wholly dependable, and that he was getting satisfactory results in using Sargent. I do not know how anything could be more contradictory to any such assertion, than what I have just read from Udy himself, as a summary of what was his experience during that year.

Moreover, from the Udy note books and reports, there have been put in evidence lists of different things which he plated. Now some of that plating was done, so the defendants said, with a Sargent formula. Well, the answer to that is that at one time Mr. Clark, in asking Mr. Udy about some of these things, wanted to know whether a certain article was plated with a solution that was supposed to be free from sulphate, or one that was supposed to be in accordance with the Sargent formula, and Udy did not know. The truth of the matter is he did not know, because of his erroneous analyses, just what his solution was at any time. So I think we can dispose of that in this way, by saying that so far as Udy attempted to follow Sargent, he simply is another one of those which this Court and the Court of Appeals found could not do so, and which caused them to say that Sargent was anything but dependable.

Now, we come to the second phase of Udy's work. In November, 1923, using not Sargent's solution — he had gotten thoroughly fed up with that, apparently Udy started off with a bath made up of what he thought was his pure chromic acid; that is the acid which had been treated by this barium hydroxide precipitation method; and he coated anodes with sulphate bearing material, and found to his surprise, that he got plating; and the high water mark of his knowledge — I think I am quoting now from the defendants own brief, was in a statement which he made in one of his reports of March 1924, that he had found that chromic sulphate is therefore an unnecessary addition to the electrolyte; it is merely a method of adding H_2SO_4, which is sulphuric acid, to the electrolyte in that it contains the sulphate radical.

That was the high water mark of his knowledge; and the high water mark of his tests on which our friends rely, was the plating of 400 or 500 strips of graphite. There is some question as to what size they were, but that does not make much difference. They were plated as was shown, for a subsidiary of the Electro-Metallurgical Company for some experimental work they were doing, and they were sent out to Indiana after being plated. The bath was made up with chromic acid, using the barium hydroxide precipitation method, which, as you will remember, does not account, even theoretically, for radicals other than SO_4.

Now, Udy made one analysis of the bath, at the beginning, after he had made it with sulphuric acid, as I recall. He plated those graphite pieces for a period of two weeks. It took him two weeks to finish them; and during that time the only additions which he made to his bath, were water and this so-called pure chromic acid. He made no other analyses; he had no knowledge of what the bath was after the initial analysis, assuming that that initial analysis was correct. We have no proof here as to when it was that his method of analyses became correct.

As your Honor can understand, unless there is some outstanding thing which some one does, how can one tell whether the method has been corrected or not? There was put into this case a question and answer of Udy, that I must confess does not appeal to me, but I may be mistaken. Mr. Clark called the attention of Mr. Udy to the fact that a certain string of analyses made from day to day in one of his experimental runs, appeared to be more uniform than heretofore. "Yes," Udy said, "that was when we were beginning to ascertain how to make our analyses." Well, I can not see what uniformity in result could have to do with accuracy of analysis. If that be true, then the most accurate results would be the ones just before that time, where Udy calmly put down from day to day the same SO_4 content, without making an analysis; and there he had absolute uniformity. If one would judge from that uniformity, that his methods of analyses were perfect one would be mistaken, for they were

nothing but guesses. So that I say we have no way of knowing when Udy's methods of analysis became correct or were corrected; so that we do not know actually even the SO_4 content of the bath which he used in plating these graphite strips; and, we certainly do not know what other radicals there may have been in it; and, there was no regulation of the bath in the sense of analyzing and restoring the chrome and acid radical. All that was done was to put in from time to time, as Udy recollects, (and there is no record of it) this so-called purified chromic acid.

The high water mark of his proposals of method is found in his abandoned patent application of June 1924. Udy agrees that that sets forth the process which he had developed; and we can concentrate on that, I think, for our purposes.

In that application which subsequently became abandoned, Udy said that he was proposing to do chromium plating from an aqueous solution of sulphuric acid containing a controlled quantity of chromic acid; and then he goes on to describe how to mix his bath taking commercial chromic acid and treating it with a hydroxide of an alkaline earth metal, which is the same thing as using hydroxide of barium; and he maintains his bath by adding this purified chromic acid; and in the second claim which he filed in his original application, the last element was—

"and maintaining the composition of the electrolyte by additions free from sulphate radical."

Now we propounded to Professor Stevenson the question — what would happen if one attempted to carry out that process commercially? At page 1412, you will find his answer, and I am going to take time to read it, if I may. I was asking whether that would result in a commercially practical method, and he said, "No", for the following reasons:

"In the supposed case, the use of perfectly, absolutely pure CrO_3 in water solution, if it were used to add to the bath to replenish the chromium that is plated out, then in the long run, in view of the fact that there is some spray loss in any sort of chromium plating process, and any process where there is a great deal of drag-out, there is still more loss of the electrolyte, and of course even where there is very little drag-out, there is some loss of the electrolyte.

"In view of that fact, the addition of merely pure CrO_3, while replenishing the CrO_3 would in no way replenish the rest of the electrolytic bath, to wit, the sulphate that is there, or sulphuric acid. That is, of course, a suppositious case, since it is based upon the assumption of a perfectly pure chromic acid solution, which is not a thing that has a counterpart in reality. If we then come to the real situation, and take a chromic acid solution used for replenishment — that is made by the precipitation of the sulphate by means of a barium hydroxide solution, which is the best precipitant that I could recommend, in this circumstance, and is the one in the question, then it is exceedingly difficult in practical operation to precipitate all the SO_4 from that solution, and yet not have an excess of barium. That is an exceedingly delicate matter. You are very apt to have an excess of either barium or SO_4 remaining at the end. Now, in the case of the barium remaining at the end, then as this chromic acid solution is added to the bath, the barium precipitates from the bath some of the SO_4, thus maintaining the depletion of SO_4 in the bath."

Let me pause a moment there, and see if I can make clear to your Honor what the Professor means there.

Theoretically, of course, with a known quantity of SO_4 in your chromic acid, if you put in the correct theoretical amount of barium hydroxide, the two will just combine and become zero, so far as the sulphate is concerned, that is to say, the SO_4 precipitates out.

That, experience has shown, is a delicate matter, a difficult matter, an impossible one from a practical standpoint. So that there are two things that you can do; one is not to put in so much of that barium hydroxide, so that you have a remainder of the SO_4; or you can put in an excess of barium hydroxide which will insure removing all the sulphate; but then some of your barium remains

in that chromic acid. If I want to replenish my bath with that chromic acid, with excess barium in it, and I put it into my bath, the barium reacts with the sulphate that is already in my bath, and depletes it just the same as it did originally when I put the barium into the chromic acid itself. That is what Professor Stevenson is talking about. You either have some sulphate left, or barium, and if you have the barium, that depletes the sulphate already in the bath. Then Professor Stevenson goes on to point out why the method takes no account of other acid radicals in the chrome acid or radicals which may come into the bath through the dip, and what-not, all of which have to be taken care of in regulating the bath; and from that he reaches the conclusion that "such a process would fail commercially."

That is the Udy process of his application, and is really the one which he used in plating his graphite strips.

Now, before we consider the legal effect of all this, I just want to refer to Udy's subsequent activities. He continued some desultory experimentations in chromium plating for the next year or so, but he went off to other subjects. He was interested in a number of things at Niagara Falls. For instance, one thing, as I recall he was interested in trying to make was chromic acid from chromium hydroxide, things which have nothing to do with chromium plating directly, although possibly indirectly.

There is one interesting thing, however, that I want to call your attention to, to show what the Udy process does *not* do, and that is take care of acid radicals other than the SO_4.

Now, sometime in July, I think it was, of 1924, Critchett asked Udy to produce 25 pounds of electrolytically-deposited metal, which, he said, if Udy could do it, then possibly he had some process that would be worth while; and Udy's records show he started in to try to do it, and immediately he began to get erratic results, and he fooled around with the thing until the following September, as I recall, the last part of September 1924, when he found that actually his chromic acid had in it other radicals which had to be removed before he could get any kind of results whatever such as he wanted.

Now, that illustrates that the Udy process as exemplified and set forth in the application of June, was anything but a commercially practical process.

I call your Honor's attention to the fact that all this occurred long after Fink had put his bath No. 60 into operation in the middle of May following his invention.

Now, Udy does not come out into the public in any way until May, 1926, which was a month or so after the Fink patent issued.

Then he was sent out to a place in, I think, Cleveland, to the Metals Protection Company, to install a chromium plating plant, or bath, and he did it. Did he use the Udy process? Not at all. What he did was exactly what everybody is doing today—he took the then commercial chromic acid which had a very nigh purity, and he mixed it with his sulphuric acid and proceeded to plate, maintaining his baths by the addition of sulphuric acid.

Now, bear in mind, however, that that was subsequent to the actual issue of the Fink patent which was issued in April of 1926; and of course, it was after the time Fink's Waterbury plant had started up, and it was more than a year after Center Street had been in operation; and I call attention to it merely to show how late it was before Udy did anything in a way that would be accessible to the public.

Now, Udy continued in the employ of the Union Carbide and Carbon Research Laboratories until 1931, I think it was, when, due to the depression, he was laid off; and he took away with him, as a matter of fact, copies of his records; and when he testified, as I recall, he was then in the employ of the Swan Chemical Company down in Alabama. The thing I want to emphasize is first and principally that this so-called Udy process is not the Fink invention. I do not care what other legal effect there may be as to what Udy did; and I want to call your Honor's attention to that shortly. The fact is that Udy did not have the Fink invention. One reason for my saying that, is that, of course, in order to anticipate, the two things must be alike, not only in method of operation, but in result.

Now your Honor knows that the Fink process as set forth in the five rules, certainly does give a continuous and successful method for chromium plating. Not even the defendants, so far as I know, challenge that. As against that, we have the testimony of Dr. Stevenson that the Udy process will not give a continuous commercially successful method; and we have the fact that Udy himself never practiced that method. He never, so far as I know, used the barium precipitation method; and the two differ; (that is, the Fink process and the Udy process;) first, in this barium hydroxide precipitation treatment.

Of course there is nothing like that in Fink. Fink has provided a method by which you can use either pure chromic acid or impure; it does not make any difference. He has given you the rule by which you can use either.

Our friends say that Fink's process is limited to the use of very impure acid. That is not so. What Fink has done is to give a method, which is so broad as to cover either the use of pure or impure acid.

Secondly, Udy ignored acid radicals other than the SO_4. Fink, on the other hand, distinctly tells you that you must take into account all the acid radicals that act as catalysts to get the total effect.

Third, Fink regulates for continuous operation; that is to say, he analyses and adjusts the bath from time to time to maintain the desired ratio. Udy had nothing of that sort, and did not suggest anything of that sort; in fact, he did what we have called in our briefs "deregulation", because he was relying on the addition of this so-called and supposedly pure chromic acid, which, in fact, was not pure. It might contain sulphate; it might contain barium; but whatever it did contain, it would have a deregulatory effect on the bath, and one which, of course, was outside entirely of anything which could be called controlled.

And then last, the Udy bath was uncertain at the start, and always, and bound to be so, because of the failure to take into consideration any other acid radicals than the SO_4. Even though you analyze for the SO_4, it is still left unknown and unknowable what the total acid radical catalyst content might be.

So we say that the Udy process was not an anticipation of Fink even though it was something which Udy was using in such a way as to make it legally effective for that purpose.

I may say, by the way, that our friends recognize that Udy did not regulate his bath when he plated these 500 pieces of graphite, on which they must depend for their reduction to practice. They attempted to show that Udy knew all about regulation; and they have combed his voluminous records, and found but three instances which they say show regulation.

I shall not take time to go into that, but they are referred to in our reply brief at page 10; and I will just give an example of one of them, which I think is illustrative.

For instance, in operating one of his experimental baths, he found in using chromium anodes, as I recall, he got a very great amount of trivalent chromium, and wanted to get rid of that trivalent chromium. He emptied the bath into another receptacle, and treated the solution with barium hydroxide to precipitate the sulphate. He then electrolyzed to remove the trivalent chromium, and now having gotten what he thought was what he wanted, pure chromic acid again, he put it back into his bath, and then re-added his sulphate. In other words he made up a new bath. He drew the old stuff off, and put it back, and made up a new bath; and that, they say, shows he knew all about regulation; and that is a sample of the three instances they have been able to dig out, to prove that Udy knew all about regulation.

But whether he did or not, as I will show you later, Udy certainly is not what the patent law calls prior art. That is aside from any question of anticipation, and aside from any question of his being the first inventor; but as a basis for a non-invention argument, on which they depend for some of their arguments—Udy is not prior art.

Now, the law as to Udy: Your Honor of course fully appreciates that the primary purpose of the patent law is the benefit to the public; and for that reason, the law frowns on, and certainly does not treat kindly things which do not benefit the public.

Now, in this particular case, with the abandoned Udy application, with Udy not coming to the service of the public until after the Fink patent issued, we have a situation where, as against a man who began giving his invention to the public in 1924, and continuously thereafter; we have as against that, these archives, these writings of incidents which occurred in a chemical laboratory, a research laboratory, dragged out in an effort to invalidate the Fink patent.

Now, Udy's work all was confidential; the very nature of the work he was doing was that. The research laboratories do not advertise what they are doing, of course; that is axiomatic almost; and in this particular case, we have the testimony of Beckett and Critchett, both called by defendants, who testified that all of Udy's work was experimental and research, and Beckett said that knowledge of the research work which Mr. Udy did, was kept within the organization; and Udy himself admits that is was all experimental; and that outside of this metal winning which he did, the only plating up to at least the middle of 1924, were odds and ends sent to him as test pieces.

Now, legally, Udy's writings as such, have no standing here, by way of anticipation or of prior art. They are not publications. If they had been printed, that would be a different matter; but they were not. They were writings which were kept within the bosom of the family. That same thing applies to the abandoned application. It has been stated over and over again that an abandoned application can not be used as prior art or anticipation.

So that when we look at defendant's brief where they have devoted over three or four pages in parallel columns, purporting to show the similarity of the language used in the Fink patent, and in the Udy writings we must realize that that is a legal nullity, because that is something which has been taken out of the Udy writings, which were never given publicity, and set up against the public writings of Fink.

Now, similarly, we say that all of Udy's doings were confidential and not accessible to the public. There is no proof that anybody ever saw what Udy did. As a matter of fact, even if they did see Udy's baths in operation, they could not have possibly known what the invention was. The stuff which he sent out, even those 500 plated strips, the mere fact that they were chromium plated, did not mean anything because chromium plated articles were, *per se*, old. The law which we have set out rather copiously in our brief, is all to the effect that in order for a thing to be an anticipation, it must be something which was accessible to the public. It may be accessible to a very small part of the public, but at least it must be accessible to the public.

I find my time has run in discussing the facts, and I shall not spend very much more time on the law. It is all set out in our briefs; and I think your Honor can get it from those as readily as anywhere else.

There is one point, however, that we have not touched in our brief, perhaps, and that is the citation from the Court of Appeals case, found in defendants' reply brief, page 19, on the socalled *Aero Vox* case, where Judge Hand held that in a defense of two years' public use by the patentee himself, the burden was placed on the patentee, once the use was shown by unequivocable evidence that in fact such use was secret; and our friends say that that same rule should apply to the case here, where they are setting up Udy as a prior inventor. I can see no reason why it should.

Judge Hand was very specific in the *Aero Vox* case, in calling attention to the fact that he was following an earlier Supreme Court case, where the specific defense of two years' prior public use by the inventor was the issue. I can see no reason why, when defendants set up a third party as a prior user, that the usual rule of proof beyond a reasonable doubt as to all parts of the defense, should not apply.

Now, I want to touch on one matter which defendants have exploited at considerable length in their briefs, on this Udy matter. Out of the story, they have set up a charge of what amounts to a plot, beginning back at the time of the organization of the plaintiff, for the purpose of suppressing Udy. I want to call your Honor's attention to this fact. When Mr. Gifford and Mr. Townsend had the Udy application before them, in their possession, what a wonderful temptation it was for them to allow a patent to issue on the Udy

application. Thereby they would have prolonged the monopoly on this invention, assuming that Udy's application showed it, for over a year and a half, because the Fink patent at that time had been out about a year and a half; they could have prolonged it at least a year and a half, because the 17 years for Udy had not begun to run at that time. Actually we all know they could have prolonged it longer than that if they wanted.

Besides, they would have had the advantage of a year and a half earlier application date; and yet we find, in spite of all that, that they decided to abandon Udy.

I think I have shown your Honor why it was they did so. It was because the Udy application, setting forth as it did, the Udy process, did not disclose the Fink invention. It was not a commercially practical method.

Now, defendants say, "Oh, but Messrs, Gifford and Townsend were dishonest. In fact they were afraid that if the Udy application went back from the Examiner of Interferences to the Primary Examiner, it would be rejected on Sargent and some other art possible."

In other words, they are asking that your Honor should assume, that Messrs. Gifford and Townsend feared the Primary Examiner would reject on the very art which you and the Court of Appeals held did not anticipate. Secondly, they ask you to assume that Messrs. Gifford and Townsend feared that the Examiner, after having declared the Interference, and after the Patent Office, against our protest, had insisted that Udy had a right to make the claims, would reverse himself and say that the claims were not patentable. I think that a mere statement of it shows that there could be nothing in that sort of inference.

Defendants also have elaborated at considerable length on various things which were done with pending applications.

They say that our purpose was to conceal the fact, that there ever had been this Udy Interference—of course, they can not blink the fact as they themselves admit, that the Udy Interference appears right on the file wrapper of the Fink patent; it could not be otherwise; and it is a direct invitation for everybody to follow it through if they wish. They say that abandoned applications are buried in the archives of the Patent Office. However, abandoned applications are always accessible to everybody who shows a proper reason for obtaining them. But the principal thing is, as I see it, and they overlook this entirely in their charges—that we never attempted to do anything with Udy himself. Udy walks away from our, I say from our,—the Union Carbide's employ—with the complete set of his records. Nobody ever asked him about that, or tried to seal his mouth, or tried to get him to say anything different from what he said here on the stand.

Now, if there had been this "conspiracy", I assume that the people who are supposedly charged with running it, would have had enough intelligence to know that the primary source for disclosure was Udy himself; yet we find him being laid off. Udy was not kept continuously in our employ, or anybody's employ connected with us. He was laid off in 1931, as a free lance, carrying away his records with him undisturbed. Finally, they make a great deal of the fact that when Beckett was subpoenaed to produce Udy's records, that some of those that surround the filing of the June applications, were not produced.

Now, Beckett's Company, I think owns something like 13 per cent. of the plaintiff, and frankly, I can not believe that Beckett would either at the time subsequent to this Interference, or any other time, have disposed of those papers. If he did, all I can say is, he must have been soft in his head, because what did he produce? In the first place, at that time, defendants already had a copy of the Udy abandoned application, that confessedly sets forth the entire Udy process that was developed; so that any papers they wanted could only supplement it, to say the least; and certainly they could rise no higher, as an effective thing, than the application itself. Secondly, Beckett produced Udy's original note books, which had been preserved, and those note books contained the records of the plating of those 400 or 500 graphite strips on which defendants depend.

As I say, I happen to know Dr. Beckett, and I know of his standing. I do not believe he would have been quite so soft in the head as to have suppressed such an inconsequential thing, and produce the really important things.

Now, we come to another defense, the socalled "Manhattan Defense." The Manhattan Electrical Supply Company had a plant over in Jersey City. They started in making a new form of primary battery. One of the things which they wanted was a number of rings or washer-shaped pieces to be put into the battery. It really was a dry battery, and those rings were made of some sort of a chemical composition, which was very hard on the tools. Yngve was the man in charge of the research work there. Under him was one Damon, and another by the name of Hill. Hill came along, as I recall, in January 1924, a little after they had started up their experimental work on chromium plating.

Yngve conceived the idea that he could put a layer of this chromium on the tools that would stand up; and he had Damon read the Sargent article, and Damon made up a little laboratory bath, and plated a core pin and thereupon they filed an application which went to patent. That application, however, was for a chromium plated tool, and in it, so far as the plating is concerned, he said he simply used a Sargent solution.

Now, this particular defense, in my opinion, falls very closely into the facts on which the Court of Appeals disposed of the *Westinghouse prior use.*

In the Westinghouse prior use, the Court of Appeals found, as you will recall, that they made up a great number of baths; until about the first of October, 1924 the proportion of chromium sulphate was generally, if not always, far too high; as was also the radical, though that is unimportant for the reason just given. Sometimes they had good baths, and sometimes bad ones; but the particular thing which influenced the Court of Appeals—I do not know how it influenced your Honor, because you did not discuss it but I suppose it influenced your Honor in the same way—was whether or not those people were doing things accidentally, or knew what they were doing. The real test is—could they reproduce what they had been doing?

In this particular case, we propose to show you that so far as the Manhattan Company is concerned, that they could not reproduce their results; that so far as the SO_4 was concerned, they allowed it to wander haphazardly; that there were innumerable instances where, if they had known that the acid radical was the important thing, they would not have done what they did do. The fact is they started off on the wrong foot. They had a theory as to trivalent chromium which permeates all of their activities. Our friends, the defendants, want to pick out the baths where they got successful plating, and where the SO_4 ratio was, I think, within Fink's limits.

We say that, to determine what the Manhattan experimenters did, and whether or not they really had an operative process, and whether or not they had the Fink process, you must look at their entire history. You can not just concentrate on one place rather than another.

If this Court and the Court of Appeals had done that, there would have been quite a different story in the *International* case, because repeatedly the Bureau of Standards had baths which operated at times. Eastman had baths which operated at times. Westinghouse had baths which operated at times; so that to say that the mere showing that some one had a bath which operated for a period, was an anticipation, is to fly in the face of what this Court has already done, and what has been confirmed by the Court of Appeals in the *International* case.

Briefly, the Manhattan history runs something like this. Damon made a digest of the Sargent article in December 1923, and it is rather interesting, because it shows that all he got out of that Sargent article was that the electrolyte was to be chromic acid and chromium sulphate. In other words, no idea of acid radical, no idea of what the real key to the chromium plating process as we know it today, should be.

Then we have another memorandum of his of April 24, 1924, in which he refers to the usual type of chromic acid plating solution. He says it is important to keep the proper ratio between what? There was a fine time for him to say the proper ratio between chromic acid and sulphate. Did he?

Not at all. It was the proper ratio between chromic acid and trivalent chromium, and in that particular memorandum he goes on to show how to produce the trivalent chromium, by putting in methyl alcohol. I do not know what the chemical process is, but we can accept it that any organic material like methyl alcohol put into chromic acid changes the hexavalent chromium of chromic acid into trivalent chromium. So they were going to use methyl alcohol for that purpose.

In May 1924, we have a memorandum which Yngve handed to Damon. I emphasize that because now we find, for reasons which are not difficult to ascertain, defendants referring to this as the Damon formula. As a matter of fact, apparently it originated with Yngve. At any rate the memorandum was handed by Yngve to Damon to read and see whether it was correct, and in that is this statement, and it throws great light on what occurred at Manhattan. It is reproduced at page 71 of our main brief:

> "We have found that the solutions as recommended by Sargent and other investigators, are greatly improved by the presence of more Cr_2O_3 (trivalent chromium) than is theoretically required to combine with the SO_4 radical present."

What did that mean? In chromium sulphate the theoretical combining of values means that the trivalent chromium is about one-quarter and the SO_4 about three-quarters of any given quantity of chromium sulphate. An ounce of chromium sulphate would have about one quarter of an ounce of trivalent chromium and three-quarters of an ounce of SO_4. So that when they were talking about theoretically combining with SO_4, they were talking about chromium sulphate. Now they say "We want in our baths more trivalent chromium than we would get if we would put in just chromium sulphate."

> "We prefer a solution containing 24½ per cent. of CrO_3 and about 2¼ per cent. Cr_2O_3 (trivalent chromium) and about .2 per cent. SO_4."

In other words, they were using the SO_4 there as part of their chromium sulphate, so that one could tell how much more trivalent chromium they wanted in their bath than they would have gotten from the chromium sulphate alone.

Throughout their work, they analyzed for trivalent chromium and for SO_4 and our friends say that the fact that they analyzed for SO_4 showed that they appreciated the effect of the radical. We say it does nothing of the sort. If I say to you that I want more trivalent chromium than would be obtained from an equivalent amount of chromium sulphate, the only way you can check to see whether or not your bath is in accordance with my recommendation is first to determine how much SO_4 is in it, and from that you can tell how much trivalent chromium should be there to theoretically combine with the SO_4 to form chromium sulphate and then you can tell whether or not the trivalent chromium present exceeds that theoretical amount and that was what their formula called for.

The fact that they did not think that the SO_4 for which they were analyzing, was the important thing, is shown by what happened in, as I said, a number of instances. For instance, sometime in June 1924 they wanted to ascertain the effect of varying amounts of trivalent chromium, and they set up a bath and they did vary the amount of trivalent chromium by putting in more and more methyl alcohol.

From that they recorded that when they had 2.52% trivalent chromium the plating was "flaking", with 2.26% it was "O. K."; with 1.22% it was 50 per cent. good; with 97% it was "getting worse". From that, they drew the conclusion that 2 per cent. of trivalent chromium gives the best result.

That in itself means nothing. But what was happening to their sulphate during that time? They were drawing these conclusions as to what trivalent chromium content would give the best plate, and the fact is that they were allowing their sulphate, as their own analyses showed, to wander from .2 per cent., with which they were supposed to start, up to double that amount, and back again. If they had had any appreciation that the SO_4 meant anything, of course they would have maintained it constant, because that was the only way they could have told what was the effect of trivalent chromium which they were increasing, but they paid no attention to the SO_4.

Then a year later they made some tests to see what would be the effect of various concentrations of chromic acid, and here again they paid so little attention to the effect of the SO_4 that they did not even analyze their baths for it. In other words, we do not know today whether they had much or little sulphate in it. Knowing today the Fink invention, if you wanted to ascertain the effect of varying amounts of chromic acid, various concentrations of chromic acid, the first thing you surely would do would be to see to it that the sulphate was determined and its ratio maintained.

In particular there are two baths which to our minds fully show that the Manhattan experimenters could not reproduce their results. The records show that in April 1924 they made up a bath which is called No. 8. I think it appears that it was used for three days. They got no plating out of it and apparently discarded it. They made no analysis and nobody knew or knows today what was wrong. Incidentally this occurred while Damon was directly in charge of that work.

Then, and this to my mind is most significant, in April 1925, this was a year, mind you, perhaps a year and a half after Manhattan had been working with these chromium plating methods, that they made up a new bath, bath No. 14. They made no analysis of it for five weeks after it had been in use. During that entire time it gave consistently dark plate.

Hill testified that dark plate was the result, but he did not know what was the cause of it and never did know what was the cause of it. All he knew was that it would chip off the tools and could not be used. They made an analysis on June 4, 1925, and that showed a concentration of .40 SO_4, twice the amount of the theoretical formula which Yngve had given Damon the year before. It did not make any difference to them. They paid no attention to that at all, although they were having trouble with the bath.

Three weeks later without an analysis they just blindly added some chromium sulphate to the bath, and the plating still continued dark. Five days later they gave the bath another blind shot of chromium sulphate, and then on July 2, after they had been monkeying with that bath for about three months, the bath was still plating dark they syphoned it off.

The bath was made up in the first place by Yngve, or under his direction, and the syphoning off was under his direction. They could not make a satisfactory bath, or at least they did not know what was wrong with that one. Our friends dispose of that by saying that apparently it had gotten contaminated in some way. Perhaps it had, but the trouble is that these workers did not have the slightest idea what that contamination might be.

I can suspect and I can suggest, that the contamination may have been other acid radicals, and very likely it was, but they did not have the slightest idea of what was wrong with the bath, and they did not know what to do with it, and they syphoned it off.

But to excuse a thing of that sort, our friends refer to something which came out in the testimony of our witness Burr, as to what occurred at Waterbury. The Waterbury plant was then in charge of Crowder, so far as the baths were concerned. Crowder was away on his vacation. The superintendent there sent down word to New York that he wanted some one to come up and fix up the baths since they were having trouble.

Burr went up, and there was a large number of series cells, small baths run in series, and Burr said that they were in such bad shape that the quickest thing to do, because the superintendent wanted to get them back into production as quickly as possible, was to draw off the liquid into a crock, which he did. He made up new baths, tested them, and then they resumed their work.

Defendants say that that is analogous to what occurred at the Manhattan Company. I think your Honor can see the difference between fussing with a bath for two or three months and then syphoning it off, and deliberately syphoning it off and immediately making up a new good bath merely as a matter of saving time.

It is true that the Manhattan people sometimes added chromium sulphate and sometimes sulphuric acid to their baths, but actually I can find no rhyme or reason to anything they were doing. As I say, Bath No. 14 of 1925 is em-

phatically an answer to the fact that they did not know what they were doing.

The thing which our friends rely on most heavily was an incident that occurred in July of 1924. That was in bath No. 10. They had been having trouble with it. They did not know what was wrong. After the June tests, which I have spoken of, to determine the best amount of trivalent chromium Which should be present, they began dosing the bath from day to day with methyl alcohol. They continued to get some plating which was flaky, that is to say, as Hill testifies, it was so soft you could rub it off with your fingers.

They kept dosing it from day to day with methyl alcohol. They took an analysis and the report showed only a trace of SO_4. Then a week later they took another sample for analysis, and that came back showing a trace. Two days after that second sample was taken they gave the bath a dose of chromium sulphate, apparently without any particular idea, or at least nobody could put their finger on any particular reason why they should do it, nor the quantity; and of course the matter is open to doubt, because we do not know when they got the result of that first analysis, but they certainly let that bath run along for quite some time with apparently no sulphate in it and still did nothing about it.

I strongly suspect that there was something wrong with their analysis because actually they were getting some kind of plating, something which they would not have gotten if they had only a trace of sulphate. They probably had other acid radicals in it, and did not know it. That probably is the answer to all the troubles which they had at various times when apparently they were having trouble and when apparently things ought to have been all right judging from the SO_4 figures.

Our friends say that the chromium sulphate was added after the second sample was taken, but before the results from the second sample were received. In other words, they are trying to say that the Manhattan Company operators and experimenters were so foolish as to take a sample and then, two days later, change the bath deliberately and thus vitiate any possible information that might come from the sample which was taken before.

I do not know what is the fact and nobody else knows, and the whole thing is vague and ambiguous. You can not spell out anything from this in favor of the defendants because, while it is true that chromium sulphate was added once and sulphuric acid another time and sulphuric acid again, as I say there was no rhyme or reason to it, and of course ten years after the event the men themselves do not know why they did it.

There is just one more point on that that I will speak of, and that is an attempt to lay on Mr. Hill's young shoulders the responsibility for what happened, for instance to bath No. 14, and for what happened in many other baths. The proofs show that they allowed their SO_4 ratio to wander all over the map and paid no attention to it in many cases when it was way beyond the Fink limits.

Now, our friends are suggesting that of course Damon was the man who had the key to the situation and when he dropped out, so far as the control of the baths was concerned, in the fall of 1924 as I recall, that after that the mistakes and errors of young Mr. Hill of course should not be held against the experimenters.

The fact is we foresaw something of that sort and so we brought it out in the examination of Mr. Damon and Mr. Hill both, that Yngve, Damon and Hill were the "Three Horsemen." What one knew the other knew. Damon testified that in so far as he knows he told Hill everything he knew about the baths. Yngve, of course, was the boss, and he was the man who apparently prepared this formula, and Yngve was the man who told Hill or directed Hill about the making up of bath No. 14 and how to dispose of it. And so I do not think they can hang on Mr. Hill the facts of this case which are so ruinous to the Manhattan defense as such.

To sum up as to that, we say that the Manhattan defense was exactly like Westinghouse. They did get good plating at times, and they did get in trouble at other times, and they could not reproduce their results. In fact, in this particular case I think it is even more illuminating than in the case of West-

inghouse, because at least in Westinghouse as far as I know the record, there was no case where they made up a bath that would not work at all; which they had to dump after fussing with it for two or three months, and yet that is what happened at Manhattan.

I think the Manhattan defense can be dismissed with the same comment found in Judge Hands' Opinion as to Westinghouse, that "the proof is too uncertain that they had even established any limits to the ratio in terms of the sulphate; it certainly does not disclose the idea that the radical alone was important." That was an idea that the Manhattan experimenters never had.

As to infringement. While we had to put on witnesses subpoenaed from the defendants' camp to prove infringement, as to New Departure, I find no dispute as to what those witnesses testified to. In the case of the Bassick Company we had an ex-employee and his testimony is not challenged in any way. So there is no question about what defendants do.

They make up their baths and they use commercial chromic acid and at the present time that is very good stuff. The reason it is very good stuff is because chromium plating has induced it. Chromic acid is especially manufactured now for chromium plating. This I think is of great significance. As early as 1928 we have an advertisement in the record of the Mutual Company boasting that their chromic acid is 99 3/4 per cent. pure. In other words, there is only 1/4 per cent. of anything in their chromic acid other than the chromic acid itself.

Their advertisement at that time says:

"In the light of present knowledge"—

they are honest about it—"It is quite evident that many of the baffling failures in chrome plating in the past were due to impurities, some of which need only to be present in minute traces to cause disastrous results."

If some of these alleged prior users had only known that earlier, then there never would have been a valid Fink patent, but they did not know that, and that is one reason why they were in the troubles that you have seen.

Our friends purchase this very pure chromic acid. We analysed samples of defendant's chrome acid and showed that it is quite that, that it has a very low SO_4 content, and substantially no other acid radical. I do not think there is even a trace of any other acid radical as I recall.

They then make up a solution using chromic acid, sulphate and water, and they proceed to plate in the usual way. When they come to maintenance, they analyze their bath for SO_4 and CrO_3 and nothing else, at least nothing else to which they pay attention. They did occasionally analyze for trivalent chromium at New Departure, and Loven testified that he always did that, but the SO_4 and CrO_3 are the only things which they adjusted for in the maintenance of their bath. How do they do it?

They calculate how much SO_4 there should be in the bath originally due to the amount of chromium sulphate that they put in there. From their analysis they determine whether or not that amount of SO_4 has decreased or increased. Then the difference between the two is reconverted by arithmetic into chromium sulphate, and then they add that much chromium sulphate.

They say they are restoring the amount of chromium sulphate in their baths which was originally in there. Actually we say they are doing nothing of the sort, but instead are simply restoring the SO_4 and that is the only thing they keep their eye on, and the only thing that is of any interest to them.

They say that they are following Sargent. I think your Honor will quite agree with me that that sort of a statement needs explanation in view of the history of Sargent in the hands of people who were either contemporaries of or preceded Fink. In this particular case we have seen what happened to Udy when he attempted to follow Sargent. He had to make changes and even then did not get anywhere.

So far as Manhattan is concerned, they also attempted to follow Sargent but they made changes so far as the trivalent chromium is concerned, and they had to dose their baths with methyl alcohol and yet these defendants say that they were following Sargent and getting good results.

Since all of Sargent's followers did not get good results, and defendants do,

there is a missing element in the equation somewhere, and I think it is not difficult to see that the missing element or missing link is supplied by the Fink patent in suit.

The first thing that our friends attempt to do is to say that what occurred in the *International* case on infringement, has no bearing on them; that in the *International* case the stipulated infringement did not relate to chromic sulphate, but related only to the SO_4. To show that there is nothing to that, we have reproduced in our reply brief at page 78 the stipulated steps of the *International* case, which, of course is part of the record of this Court.

Mr. Cooper: It is not in evidence here.

Mr. Scull: No, it is part of the record of this Court—we show step by step, that what the defendants do here is exactly the thing that was stipulated in the *International* case. So that I can say that if, as this Court found and the Court of Appeals affirmed, that constituted infringement in the *International* case, so it should constitute infringement here.

The defendants defend themselves by saying that they have three steps, each of which is traceable back to the prior art and not to Fink, and therefore they are not infringing. Let us examine that..

In the first place, they say that they are following the familiar Sargent formula. As previously said to your Honor, and I say it again, there is no such thing in any such sense as the defendants mean it, as a Sargent formula. If there had been, the history of chromium plating would have been quite different from what it has been. If there had been something in the Sargent article in the way of a formula, that anyone could go to, and reproduce and get a successful, continuous, certain chromium plating process, then of course there never would have been any room for the Fink invention, and your Honor never would have been bothered with this case or the *International* case; but the fact is, and the history of every one of the attempted followers of Sargent shows that Sargent did not disclose any satisfactory process or formula which anybody could read out of his publication.

And so we say in the first place that they are not following the Sargent formula, because there is no such thing. All they are doing is arbitrarily taking 245 grams of chromic acid and 2 1/2 grams of chromium sulphate, and they say that that constitutes the Sargent formula.

Second, they say that *a la* Westinghouse and *a la* Udy, they are using chromic acid which is substantially free from sulphate. I am following, by the way, my reply brief at the beginning of page 83 and going on.

I have already referred to the fact that present day chromic acid which defendants use is free from substantially all acid radicals. It certainly is of anything except a small trace of the sulphate.

They say that the Westinghouse procedure as set forth in the Court of Appeals decision is justification for what they do in this respect. They are ingenious, but I do not think there is any difficulty in seeing the fallacy in their reasoning. Judge Hand pointed out that Piersol after Sept. 1924 returned to the sellers all chromic acid which had more than one-fifth of 1 per cent. of sulphate in it.

> "To what he kept he added chromic sulphate in what he considered the proper portion. The working cards do show that the sulphate radical was calculated, but it was by way of computing chromic sulphate, which alone, in common with Sargent, he thought important. It is apparent that he had not grasped the critical facts. Had he done so, he would not have sent back the impure chromic acid; he would have supplemented it with the radical in another form."

Why was Judge Hand referring to that particular thing? Was he saying what our friends say he says, that anybody who does return chromic acid which has a sulphate beyond a certain amount, is thereby not infringing Fink? Of course, that is just nonsense.

What Judge Hand was using that particular instance for was to show that Piersol had no concept that it was the acid radical alone that was the thing to count on. Judge Hand did not say that that was the thing that distinguished

Westinghouse from Fink. In fact, the last part of that paragraph shows quite to the contrary. What he was demonstrating was that Westinghouse did not know that the radical alone was important. If they had, their story would have been quite different.

They say also that they are following Udy. Do they? I know of no barium hydroxide precipitation in anything which has appeared in either what the defendants do or what the manufacturers of chromic acid do. As a matter of fact, the manufacturers of chromic acid have testified that there is no barium hydroxide precipitation in anything they do. I have forgotten how they do make the acid so pure today, but they do not do it in the way Udy suggested.

In any event, Udy is certainly not prior art. A thing done in a corner of a laboratory is not within the prior art as I understand the Court's decisions on prior art. Prior art or prior knowledge is something which must be available to the ordinary worker in the art, and not something which is kept confidential and secret by one man.

And so in this particular case, even assuming that Udy had any idea of purifying his chromic acid so that that would be a justification for the defendant, it is out of this case, in view of the fact that it was not something which was in the public domain, something which was available to the workers in the art.

Third, we come to something which I hope I can make clear, having personally struggled with it for a long time, and having tried it on various people. I am sorry to say that I must try it because it is another case of our ingenious friends using arguments which are difficult for the layman to follow, but which are fallacious.

They say that they are maintaining their bath by analyzing for chromium sulphate. To you and to me that would seem to be a very simple thing. Here is a bath that we know has had chromium sulphate put in it, and we say to the chemist, "Tell us how much chromium sulphate is in there." He reports back a certain quantity and we say, "That is not enough," and we put in some more chromium sulphate.

But everybody agrees that that can not be done. Even Professor Willard admitted, and the defendants have admitted, over and over again, that you can not go to a chromium acid plating bath and analyze it directly for the chromium sulphate. So Professor Willard says, "the only way I know how to do it is to analyze for the SO_4 and then calculate for the amount of chromium sulphate that is there." In other words, if you had in a field some horses (trivalent chromium) and carts (sulphate) that have been driven in there as teams (chromium sulphate) and the horses and carts are mixed up together after they are unharnessed, and if I knew that only horses and carts (chromium sulphate) had been driven in and I simply went in there and counted the carts, I would know how many horses and carts there were there, and that is simple. That is the kind of a picture they are trying to give you here as to what is true of the chromium plating bath.

Let me see if I can give you what the picture really is.

In the first place, when chromium sulphate or water or sulphuric acid goes into a solution, the radicals become "ionized," as they say, that is, they are separated from each other. The SO_4 is there independently of the thing with which it was originally associated.

So that if I put into a bath some sodium sulphate and some chromium sulphate, and if even Professor Willard, (and he admits this in cross-examination) should be told how much SO_4, there is there now, he can not tell me how much sodium sulphate and how much chromium sulphate there is, because the horses and carts have become unhitched and all the carts (sulphate) have been put together here, and the black horses (trivalent chromium) and the white horses (sodium) are over there. If you count the carts alone, you can not tell how many teams of black horses and carts there are.

The fact is that in every chromium plating bath, there are groups of bases; that is various colors of horses. There is certainly always hydrogen which is a base, there is always certainly trivalent chromium which is a base, and there is frequently sodium which is a base. The curious thing is that the trivalent

chromium comes into the bath from other sources than the chromic sulphate that may be put in there. It comes from the mere operation of the bath itself. Sometimes you have only a trace.

One of the particular analysis sheets that were pulled out by Hordacum, showed only a trace of trivalent chromium. Sometimes the trivalent chromium is very high. That has nothing to do with the amount of trivalent chromium that went in there originally as part of the chromium sulphate. So that you can readily see that since the amount of trivalent chromium itself is varied, that when you analyze for the SO_4 you can not tell whether that SO_4 represents a combination with trivalent chromium to form chromium sulphate, whether it represents a combination with hydrogen to form sulphuric acid or whether it represents a combination with sodium to form sodium sulphate.

So that when these people take the SO_4 and calmly assume that it is all chromium sulphate, they are just covering up the fact that they are restoring the SO_4 in their baths, and nothing else.

What is the testimony as to that? Dr. Stevenson has said that an analysis for SO_4 is an analysis for SO_4 and nothing else. We have the testimony of Maag, a witness you have not heard of, and he has testified by deposition, and it is in our reply brief at page 89.

These questions are asked by the defendants' counsel and Maag is their witness. What does he say?

Q. "Do you know of any way of analyzing a liquid which contains, for example, calcium sulphate in solution, chromium sulphate in solution, and sulphuric acid? When I say a liquid, I mean a solution—to determine the percentage of each in that solution.

A. "We analyze for the various radicals."

By the "radicals" he means not only the SO_4, which is one radical, and that is an anion, but also in this particular case, the calcium and the trivalent chromium and the hydrogen of the sulphuric acid, would be the cations.

"and then it is possible hypothetically to balance them up the way you wish, but you can not say, to my knowledge, how much is sulphuric acid and how much is calcium sulphate, and so on, exactly."

They press him a little further. He is their witness.

Q. "Could you not in a chromic acid bath give with a reasonable degree of accuracy the percentage of sulphuric acid which might be in the bath, the percentage of sodium sulphate which might be in that bath, and the percentage of calcium sulphate which might be in that bath?

A. "No."

Also in Udy's writings themselves, when he was discussing this new discovery, that you did not have to use chromium sulphate, he speaks of the non-existence of chromium sulphate in solution. He says chromium sulphate can not exist under the conditions of operation, and it does not exist as such in a solution of chromic acid. So that according to Udy (defendants say that Mr. Udy is quite a chemist) when defendants are analysing for chromium sulphate, they are analyzing for a non-existent thing and yet they use SO_4 determination to calculate what Udy says is a non-existing thing.

We have also the testimony of Wynne. Wynne testified in the *International* case, and his testimony is stipulated here. What did Wynne do when he wanted to ascertain chromium sulphate? He was sincere, and he thought that chromium sulphate was necessary to be in the bath, and what did he do? Did he ignore the trivalent chromium as defendants do? Not at all. He says that he analyzed for both trivalent chromium and SO_4, and he took what was the nearest approximation he could make. I think he was wrong, but nevertheless this is what he did, and certainly it is different from what the defendants do—he took either the trivalent chromium or the sulphate, whichever was the smaller as the basis for his calculation for chromium sulphate.

Here is a field of horses and carts. I can not have more teams than the smallest number of those articles that are there. In other words, if I have ten horses and fifteen carts, I can not have more than ten horses and carts. On the other hand, if I have ten carts and fifteen horses, I can not have more

than ten horses and carts. What Wynne did in his approximation was to take into consideration the trivalent chromium in an attempt to balance it up with the SO_4. Defendants do nothing of that sort.

The proofs show that what they are doing here is analyzing for the SO_4 and adjusting their baths with the SO_4, and the reason they are doing it is because they know now, thanks to Fink's teachings, that the acid radical is the important thing, the thing to keep their eye on.

The next thing on non-infringement is this: They are trying to borrow from the prior art, but they can not put their finger on a single prior art description or use of any sort which is just what they are doing. They have to patch together at the very least a number of things.

For instance, do they follow Sargent? I think we have said enough to show that they do not. They could not possibly. Sargent never analyzed his baths, and he never knew there was such a thing as an acid radical involved in the operation. Sargent certainly never did a thing to tell his followers that they should look out for the acid radical that came into the bath through the chromic acid. They are not following Sargent. They are trying to hide behind him, but they can not succeed.

Similarly they say that they are following Udy. Udy is not prior art that is available to them in any event. It is not a thing which they can use to show how Sargent should be readjusted, and certainly they are not following Udy. They do not use any barium hydroxide precipitation. They are not regulating their bath merely by adding barium hydroxide treated chromic acid. They are maintaining their baths by analysis for SO_4 and by putting in sulphuric acid or chromium sulphate to adjust that SO_4.

By the way, I mentioned sulphuric acid, and I want to go back for just a minute. One of the things which shows the falsity of defendants position as to the maintenance of their bath, is the fact that both defendants, New Departure and Bassick, use either sulphuric acid or chromium sulphate indiscriminately in maintaining their baths. There is not a single suggestion in Sargent,—they try to figure out one which I have not time to go into,—but there is not a single suggestion in Sargent that you were to use sulphuric acid in your baths. In fact, we have Richardson on the record saying that the one thing you should not use was sulphuric acid, and Richardson was a reader of Sargent.

When these people, therefore, analyze for SO_4, and then use either chromium sulphate or sulphuric acid to adjust their baths, is it not almost indisputable that the only thing they are interested in, is the SO_4, because that is the only thing that is in common in chromium sulphate and sulphuric acid. It is the SO_4 that they are interested in. Loven said that when he used sulphuric acid he takes into consideration the difference in the amount of SO_4 that there is in sulphuric acid as compared with chromium sulphate.

As I say, they are not following Udy and they are certainly not following Westinghouse. They are trying to pick out of Westinghouse something which they can pin on Sargent.

Anything that Westinghouse did that was successful, was an accident; and consequently, if they are following Westinghouse, they are trying to take one of Westinghouse's "accidents" and use it as a precedent, and that, as your Honor knows perfectly well, the law does not permit.

As we said in the last page of our rejoinder, and this is my conclusion, that if any one attempted to follow Sargent or Schwartz, or Udy or Westinghouse, or Eastman, and happened to get the Fink ratio, it would obviously have been a pure accident, and that this Court knows very well, under the well-settled decisions, can never justify an infringement.

We have published these reports from a copy of the Official Court Stenographer's Notes. All errors, if there are any, that changes the sense of the Arguments are due to following the report as received.

American Electro-Platers' Society

The 22nd ANNUAL CONVENTION

June 11, 12, 13, 14
1934

Detroit, Michigan

Headquarters STATLER HOTEL

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T. C. EICHSTAEDT, *Secretary-Treasurer*

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HARTFORD-CONNECTICUT VALLEY BRANCH

The Hartford-Conn. Valley Branch held a meeting on Monday evening, March 26, at the home of Mr. W. H. Stoddard of 51 Monroe St., Hartford, Conn. The meeting was conducted by Past Pres. Jas. Beloin. Minutes of the previous meeting were read and accepted. All communications were read and placed on file.

The meeting was well attended, twenty members and visitors being present.

It seemed good to see Mr. MacFadyen back again after his operation and extended stay in the hospital. He is getting along fine, though slowly.

After the business meeting, a delicious lunch was served and thoroughly enjoyed by all.

It was voted to hold our next meeting on Monday evening, April 23, at the home of Mr. Jas. St. Pierre, 120 Judd St., Bristol. We urge all members who can possibly do so, to be present.

V. E. GRANT, *Sec'y.*

CLEVELAND BRANCH

The monthly meeting of the Cleveland Branch was held on April 7th at Carter Hotel. Meeting was called to order by Pres. H. J. TerDoest at 8:30 P.M. The minutes of the March meeting were read and approved.

Our librarian, Mr. Thompson, read a paper on good management; after a brief discussion, Mr. Thompson was given a vote of thanks. Mr. Lyons then presented a very interesting three-reel movie entitled "Metal in the Fine Arts." It was very educational and the entire Branch joined with our President in giving Mr. Lyons a hearty vote of thanks for this and his past work, although he is only an associate member he is a very active worker for the Branch.

W. D. SCOTT, *Sec'y.*

MILWAUKEE BRANCH

The educational meeting and smoker held on April 7th was the best attended, and most enthusiastic meeting ever held by Milwaukee Branch. The attendance was double of what we had expected; where they all came from we do not know, but our register shows a large delegation from Chicago and its suburbs, also from Kenosha, Racine, Burlington, Madison, Beaver Dam, LaCrosse, Sheboygan, Manitowoc, Two Rivers, also from St. Paul, Minneapolis 400 miles from Milwaukee.

Geo. Hogaboom, first speaker of the evening was given a rousing reception. George held his audience spell bound with his talk on the results of the recent exposure tests of electroplated coatings, the enthusiasm as shown after he finished his talk, proves what a great benefit this research work will be to the plating industry. A rousing vote of thanks by the entire audience proved that the talk had made many friends among those interested in the plating industry.

H. A. Gilbertson, Supreme Secretary, was the next speaker. His talk on the benefits of membership in the A. E. S. ought to bring in a number of applications. He was given a noisy ovation when introduced as our next Supreme President.

Gustav Soderberg of the Udylite Process Co., talked on the Developments of Cadmium Plating; this brought him many friends, also many

questions from the audience that kept him busy for a long time.

The educational meeting was followed by a buffet lunch with plenty of the stuff that made Milwaukee famous, also a pleasing floor show.

Credit for the great success of the evening's entertainment must be given to Pat. Sheehan, master of ceremonies; he certainly put in the punch that kept the crowd in good humor. The committee in charge of all arrangements certainly did a good job as was evidenced by the great success of this meeting.

Chef Jack Geisman prepared the lunch and had charge of the refreshments. Dan. Wittig and Bob Steuernagel, the veterans of the committee, do not need applause; the huge success of this affair showed the results of their good planning and preparation. Pres. Paul Krause opened the meeting and welcomed the guests.

FRANK MARX, *Sec'y.*

BRIDGEPORT BRANCH

The April meeting of the Bridgeport Branch was held at the Stratfield Hotel in the Chamber of Commerce Room on Friday, April 6.

Ray O'Connor, Chairman of the Educational Committee announced that arrangements had been made for an Open Meeting to be held in the rooms of the Dictaphone Company on Friday, April 20. Dr. L. Klinkenstein, a chemist of Maas & Waldstein, will be the speaker for the evening.

The Bridgeport, Waterbury & Hartford Branches are planning to hold a joint banquet in Hartford in the latter part of May. The following members were asked to serve as a Committee from the Bridgeport Branch with the other branches: Ray O'Connor, Chairman; George Wagstaff, John Oberender George Knecht.

Elections of officers will take place at the May meeting and selection of delegates for the convention.

WM. F. FLAHERTY, *Secy.-Treas.*

NEW YORK BRANCH

Our February 9th meeting was turned over to Good and Welfare comparatively early, and proved very interesting. The discussion was renewed on the advisability of cadmium plating direct on brass.

It was the experience of some of the members that the cadmium plate entirely disappeared in a short time when exposed to the atmosphere. Their opinion was that the cadmium and brass caused an electrolytic action, which destroys the cadmium. To overcome this the work was nickel plated first. Others of the old time platers claimed they experienced no such trouble. The secretary would appreciate opinions on this subject from members of the Society.

Information was asked some time ago for the formula of a dip or bath which would impart a gold color to silver plate, without using gold or its salt. As it went unanswered, possibly a member of another branch knows of it and would be kind enough to forward it.

At this meeting we were honored by the presence of Mr. Horace Smith of Newark Branch. The purpose of his visit was to acquaint us with the arrangements made by Newark Branch for their Annual Affair. Judging from their list of speakers, which include Mr. Charles Proctor, our founder, Mr. George Gehling, Dr. Klinkenstein, Mr. S. Taylor and Mr. Oliver Sizelove, all recognized authorities, there is no doubt that the afternoon session will prove highly educational. It is needless to say anything of the evening Social Affair, for anyone who has ever attended any of Newark's Affairs has never failed to have a good time. Mr. Smith was assured of a sizable representation from New York Branch.

Our March 23rd meeting was very well attended due to the scheduled paper to be delivered by Mr. Henry Levine, on Preparation of Castings for Plating. Mr. Levine displayed quite a few samples of large plated castings of bronze, copper, nickel, cadmium, and chromium, and went into detail of the preparation of castings for different classes of work. Mr. Levine's paper was very well delivered and he was given a rising vote of thanks which he very well deserved. It wasn't so long ago that almost everyone in the industry had his own secret formula for everything and anything he gave out was with a great deal of reluctance. It is men of the calibre of Mr. Levine who help to keep the American Electro-Platers' Society in the high position it holds today in the plating industry.

Our distinguisehd brother, "Bill" Voss was present at this meeting, with his dry wit, and enlightening remarks. When Brother Bill is present we are always assured of just enough humor to make the meeting truly enjoyable.

It is encouraging to the officers and our Good and Welfare committee to see so many new faces at the meetings. It shows that the members are doing their part to help promote the aims and objects of the Society.

ARTHUR WALLACE, *Rec. Secretary.*

DETROIT BRANCH

A regular meeting of the Detroit Branch, American Electro-Platers' Society, held at the Statler Hotel, on Friday evening, April 6, 1934, at 8:00 P.M., with Mr. Hansjosten, President, in the chair. A very large attendance was present. The minutes of the last meeting were read and approved. The application for membership of Mr. Chester Marshall, reported on favorably by the Board of Managers, was received and he was unanimously elected to membership.

A letter from Milwaukee Branch was read and placed on file. A letter from T. F. Slattery, Chairman of the Educational Committee of the Supreme Society, was also read and placed on file.

The Entertainment Committee chairman, Mr. Cherry; the Finance Committee chairman, Mr. McCord; and the Educational Committee, Mr. Eldredge; all reported that the Convention business was progressing wonderfully and that the program had practically been closed and going to press, and as far as the success of the Convention— it looks very favorable.

Mr. George Hogaboom, of New Britain, Conn., was then introduced by the President as speaker of the evening. His talk was an explanation of his observations of the exposure tests being conducted by the Bureau of Standards in Washington, D.C., assisted by an Associate of the American Electro-Platers' Society during the last two years, and quite a few facts generally understood by many platers were proved to be true, although it took two years to make this proof. Mr. Hogaboom also made quite a few statements regarding the continuation of this research work and its financing up to now has been carried on by the A.E.S. His talk was highly educational and interesting, and a few specimens of the plates that were actually in the exposure tests were evidence and proof of the effects of the weather on electroplated steel articles. It brought out quite a little discussion and many questions after it was all over. A rising vote of thanks for his talk was given Mr. Hogaboom and also very enthusiastic applause.

Registration fee for Convention is $6.00 for Men and $4.00 for Ladies.

The following names were added to the Reception and Registration Committee: Charles Beaubien, Carl Ken-

nedy, Geo. L. Nankervis, B. F. Lewis, T. C. Eichstaedt.

T. C. EICHSTAEDT, *Secretary-Treasurer.*

ANDERSON BRANCH

The Anderson Branch met at the Y. M. C. A. at 7:30 p.m. April 7, 1934. The Secretary was instructed to write the Detroit Convention Committee that a paper and displays would be available from this Branch.

The President appointed the following committee to arrange for the annual Spring Dinner: E. A. Kaegi, G. S. Cole, C. D. Cleaver, Fred Carl, Guy Chaney.

The speaker of the evening was Mr. Oscar Gebhart of Delco-Remy Corp. who gave a very interesting talk on Plastic Moulding. G. M. COLE, *Secy.*

CHICAGO BRANCH

The regular monthly meeting of Chicago Branch was held April 14, 1934 at the Atlantic Hotel. President E. G. Stenberg presiding and all other officers present. About 50 members of Chicago Branch made the trip to Milwaukee Branch Smoker and all voted that they had a very good time and were well repaid for the trip.

Several members spoke on the test being made by the Bureau of Standards and the Research Committee, and shown at Milwaukee by Mr. G. Hogaboom. Having the samples to show the members as well as the slides renewed more interest in the research work. Chicago Branch feels that the research work should be carried on to the finish.

The meeting was then turned over to the Librarian. A very interesting paper was read by Mr. F. Strock, published by our National Educational and Program Committee.

The following questions were found ing the question box:

No. 1. *What is the method of plating chrome direct on high luster buffed Zinc base die casting? Is this method practical? How will it stand up? Parts are small die castings for household use. Will they come bright from the tank?*

Answer. It is possible to plate die casting direct in chrome, but is not the best method and is not recommended.

No. 2. *Is there any bright dip for Zinc base die casting to produce a white uniform color on the casting?*

Answer. No dip known.

No. 3. *What Nickel anodes are best for plating steel parts in warm nickel solution. Parts require heavy deposit which is buffed to a high lustre?*

Answer. 99 Anodes thought best, but good results can be had from 95-97.

Next meeting is the election of officers. All members are urged to attend.

Chicago Branch will hold a booster meeting on June 2nd.

J. W. HANLON, *Secy.*

NEWARK BRANCH

The Newark Branch held its annual Educational Session at the Newark Elks Club, Saturday April 7, 1934, with an attendance of 163. The speakers of the afternoon were Mr. Charles Proctor, subject: Chromium Plating on Sheet Zinc for the Automotive Industry. Dr. G. Klinkenstein of Maas & Waldstein, subject: Novelty Finishes. Mr. George Gehling, of Philadelphia Branch, subject: Barrel Plating. Mr. Taylor, Newark Branch, subject: Metal Etching. Question Box conducted by Mr. Edward W. Faint, Newark Branch.

With this superb list of speakers one can readily realize that those who attended heard things of practical interest to all. Mr. Proctor's fine discourse on Sheet Zinc was well received. Dr. Klinkenstein gave a very interesting talk on Lacquer Finishes and displayed samples of each product or finish and the way to procure the same results. Mr. S. Taylor gave an unusual and fine talk on etching relief by first plating a metal on another

and then etching the design on the background which would be the base metal. Our old friend from Philadelphia, George Gehling, has all the old "pep" and gave an eloquent discourse on Barrel Plating. Mr. E. Faint conducted the Question Box as well as the meeting as only Mr. Faint can. GEORGE REUTER, *Secy.*

ST. LOUIS BRANCH

The regular meeting was held on Friday, April 13th at 8 p.m.

It was with deep regret that we learned of the death of our dear friend, Ed. Heil. No doubt all members will be surprised as he died rather suddenly. Mr. B. Daw of St. Louis Branch attended the funeral. Motion was made and seconded that a letter of condolence be sent to the family.

Our next meeting will be held at the Central Y. M. C. A. on May 11, 1934, at which time a dinner will be served at 6:15 p.m. This meeting should be well attended as there will be the election of officers and delegates to the convention.

CHAS T. MCGINLEY, *Secy.*

WATERBURY BRANCH

Friday, the 13th, did not prove to be such an extra good night, for the regular monthly meeting of Waterbury Branch, A. E. S. President Tennant Elwin presided over what proved to be a rather interesting, but slimly attended meeting. Minutes of previous meeting were read and approved after which several communications were made and assigned to committees for action. Assistant Librarian Ellsworth Candel gave an interesting account of the recent conference on electro-plating, held in Washington, D. C.

Ralph H. Lee contributed some interesting remarks on matters of general interest. Waterbury's delegation to the Newark Branch Educational Session (afternoon and evening) gave an interesting account of the proceedings. It was interesting to learn that George Gehling, in his talk on "Barrell Plating" was at his best.

The committee in charge of the next meeting, which takes place Friday evening, May 11th, have expressed a desire to see every member of the Branch present, as business of importance including the election of officers and delegates to the Detroit convention will take place.

W. F. GUILFOILE, *Secy.*

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APPLICATIONS

W. T. Donnelly, 3107 S. Wabash Ave., Chicago, Ill. (Assoc.)	Chicago
Steve Gerloski, 4210 S. Mozart St., Chicago, Ill.	Chicago
Chester Marshall, 2432 Plainview Ave., Detroit, Michigan.	Detroit

ELECTIONS

W. T. Donnelly, 3107 S. Wabash Avenue, Chicago, Ill. (Assoc.)	Chicago
Steve Gerloski, 4210 S. Mozart St., Chicago, Ill.	Chicago

REINSTATEMENT

H. C. Jaeger, 2208 N. 8th Street, Milwaukee, Wisconsin.	Milwaukee

TRANSFER

Geo. Dupernell from Detroit Branch to	Waterbury
Fred Matts from Newark Branch to	Cleveland

DEATHS

Harry Flannigan	Newark	Fred Haushalter	New York

RESIGNATIONS

Henry Bergfels	Newark	F. S. Bruen	Newark

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Under the Auspices of

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